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EDITORIAL.

ITEMS AND NOVELTIES.

Bessemer Steel Car Axles.—We subjoin a tabular statement of some very interesting tests lately made by Messrs. Algernon and Percival Roberts, at their establishment, the "Pencoyd Iron Works, near Norristown."

These gentlemen had undertaken the manufacture, for the Pennsylvania Central Railroad, of Bessemer Steel Axles, guaranteed to stand the test of five blows from a ram weighing 1,640 pounds, and falling twenty feet, the axle being reversed after each blow.

To secure this test, each axle is made four feet longer than it is required to be in use, so that a trial piece of this length may be cut off and subjected to the stated number of blows.

Wherever the trial piece fails, the corresponding axle is rejected.

This method of testing is severe and expensive, yet it seems to be the only one which will insure absolute reliability with a mate-

rial like this, where uniformity in the product of manufacture has not yet been secured.

These tables, themselves, need no further explanation, as they speak for themselves, and are so arranged that their various relations are evident on mere inspection.

Trial of transverse strength of Bessemer Steel Car Axles, made at Pencoyd Iron Works. The deflection on the following tests is measured on a chord of four feet.

Diam. of Axle.	No. of Blow.	Weight of Ram.	Height of Fall.	Distance bet. Bearings.	DEFLECTION. Before Blow.	After Blow.	Effect of Blow.
4⅛ in.	1	1640 lbs.	15 ft.	3 ft.	— 00 in.	⌣ 4 in.	4 in.
	2				⌢ 4	— 00	4
	3				— 00	⌣ 3¼	3¼
	4				⌢ 3¼	⌣ ¼	3½
	5				⌢ ¼	⌣ 2¾	3
	6		25 ft.		⌢ 2¾	⌣ 2¾	5½
	7				⌢ 2¾	⌣ 4¼	7
	8				⌢ 4¼	⌣ 1¼	5½
	9				⌢ 1¼	⌣ 4¾	6
	10				⌢ 4¾	⌣ 1	5¾
	11				⌢ 1	⌣ 4½	5½
	12				⌢ 4½	⌣ 1½	6
	13				⌢ 1½	⌣ 4¾	6¼
	14				⌢ 4¾	⌣ 1	5¾
	15				⌢ 1	⌣ 4¼	5¼
	16				⌢ 4¼	⌣ 1	5¼
	17				⌢ 1	⌣ 2	3
	18				⌢ 2	Broke,	
						Total Deflection,	85½ in.
4⅛ in.	1	1640 lbs.	15 ft.	3 ft.	— 00 in.	⌣ 3¾ in.	3¾ in.
	2				⌢ 3¾	— 0	3¾
	3				— 0	⌣ 3½	3½
	4				⌢ 3½	— 0	3½
	5				— 0	⌣ 4	4
	6		25 ft.		⌢ 4	⌣ 2¼	6¼
	7				⌢ 2¼	⌣ 2¼	4½
	8				⌢ 2¼	⌣ 2¼	4½
	9				⌢ 2¼	⌣ 3½	6
	10				⌢ 3½	⌣ 2	5½
	11				⌢ 2	⌣ 3½	5½
	12				⌢ 3½	⌣ 1¾	5¼
	13				⌢ 1¾	⌣ 3½	5¼
	14				⌢ 3½	⌣ 1¾	5¼
	15				⌢ 1¾	⌣ 3¼	5
	16				⌢ 3¼	⌣ 2	5¼
	17				⌢ 2	⌣ 3	5
	18				⌢ 3	⌣ 2½	5½
	19				⌢ 2½	⌣ 3	5½
	20				⌢ 3	⌣ 1¼	4¼
	21				⌢ 1¼	⌣ 3	4¼
	22				⌢ 3	⌣ 1¼	4¼
	23				⌢ 1¼	Broke.	
						Total Deflection,	106¼ in.

4⅛ in.	1	1640 lbs.	20 ft.	3 ft.	— 00 in.	⌣ 5½ in.	5½ in.
	2				⌒ 5½	⌒ ½	5
	3				⌣ ½	⌣ 5½	5
	4				⌒ 5½	⌣ 1	6½
	5				⌒ 1	⌣ 4¾	5¾

Trial discontinued. Up to standard required by Penn'a R. R.

4⅛ in.	1	1640 lbs.	20 ft.	3 ft.	— 00 in.	⌣ 5½ in.	5½ in.
	2				⌒ 5½	— 00	5½
	3				— 0	⌣ 5	5
	4				⌒ 5	⌒ ½	4½
	5				⌒ ½	⌣ 4½	4

Trial discontinued. Up to standard required by Penn'a R. R.

4⅛ in.	1	1640 lbs.	20 ft.	3 ft.	— 0 in.	⌣ 6 in.	6 in.
	2				⌒ 6	⌒ 1½	4½
	3				⌣ 1½	⌣ 6	4½
	4				⌒ 6	⌒ 2½	3¾
	5				⌣ 2½	⌣ 6½	4

Trial discontinued. Up to standard required by Penn'a R. R.

4⅛ in.	1	1640 lbs.	20 ft.	3 ft.	— 0 in.	⌣ 5 in.	5 in.
	2				⌒ 5	— 0	5
	3				— 0	⌣ 2½	2½
	4				⌒ 2½	⌣ 1½	4
	5				⌒ 1½	⌣ 2⅝	4⅛

Trial discontinued. Up to standard required by Penn'a R. R.

4⅛ in.	1	1640 lbs.	20 ft.	3 ft.	— 0 in.	⌣ 5½ in.	5½ in.
	2				⌒ 5½	Broke.	

4⅛ in.	1	1640 lbs.	20 ft.	3 ft.	— 0 in.	⌒ 5 in.	5 in.
	2				⌒ 5	Broke.	

4⅛ in.	1	1640 lbs.	20 ft.	3 ft.	— 0 in.	⌣ 5¾ in.	5¾ in.
	2				⌒ 5¾	⌒ ½	5¼
	3				⌣ ½	⌣ 5½	5
	4				⌒ 5½	⌒ 1½	4
	5				⌣ 1½	⌣ 5½	4

Trial discontinued. Up to standard required by Penn'a R. R.

4⅛	1	1640 lbs.	20 ft.	3 ft.	— 0 in.	⌒ 4 in.	4 in.
	2				⌒ 4	Broke.	

4⅛ in.	1	1640 lbs.	20 ft.	3 ft.	— 0 in.	⌣ 4¾ in.	4¾ in.
	2				⌒ 4¾	⌣ 1	5¾
	3				⌒ 1	⌣ 3¾	4¾
	4				⌒ 3¾	⌣ ¾	4¼
	5				⌒ ¾	⌣ 2½	3¾

Trial discontinued. Up to standard required by Penn'a R. R.

$4\frac{1}{8}$ in.	1	1640 lbs.	20 ft.	3 ft.	— 0 in.	⌒ 5 in.	5 in.
	2				⌒ 5	⌒ 1	6
	3				⌒ 1	⌒ 3	4
	4				⌒ 3	Broke.	
$4\frac{1}{8}$ in.	1	1640 lbs.	20 ft.	3 ft.	— 0 in.	⌣ 5 in.	5 in.
	2				⌒ 5	— 0	5
	3				— 0	⌣ $4\frac{1}{2}$	$4\frac{1}{2}$
	4				⌒ $4\frac{1}{2}$	— 0	$4\frac{1}{2}$
	5				— 0	⌣ $4\frac{1}{2}$	$4\frac{1}{2}$
Trial discontinued. Up to standard required by Penn'a R. R.							
$4\frac{1}{8}$ in.	1	1640 lbs.	20 ft.	3 ft.	— 0 in.	⌣ 5 in.	5 in.
	2				⌒ 5	⌒ $\frac{1}{4}$	$4\frac{1}{2}$
	3				⌣ $\frac{1}{2}$	⌣ 5	$4\frac{1}{2}$
	4				⌒ 5	⌒ $\frac{1}{2}$	$4\frac{1}{2}$
	5				⌣ $\frac{1}{2}$	⌣ $4\frac{1}{2}$	$4\frac{1}{2}$
Trial discontinued. Up to standard required by Penn'a R. R.							
$4\frac{1}{8}$ in.	1	1640 lbs.	20 ft.	3 ft.	— 0 in.	⌒ $5\frac{1}{2}$ in.	$5\frac{1}{2}$ in.
	2				⌒ $5\frac{3}{4}$	Broke	
$4\frac{1}{8}$ in.	1	1640 lbs.	20 ft.	3 ft.	— 0 in.	⌣ $4\frac{3}{4}$ in.	$4\frac{3}{4}$ in.
	2				⌒ $4\frac{3}{4}$	— 0	$4\frac{1}{4}$
	3				— 0	⌣ $4\frac{1}{4}$	$4\frac{1}{4}$
	4				⌒ $4\frac{1}{4}$	⌒ $\frac{1}{2}$	$3\frac{3}{4}$
	5				⌣ $\frac{1}{2}$	⌣ $4\frac{1}{2}$	4
Trial discontinued. Up to standard required by Penn'a R. R.							
$4\frac{1}{8}$ in.	1	1640 lbs.	20 ft.	3 ft.	— 0 in.	⌣ 5 in.	5 in.
	2				⌒ 5	— 0	5
	3				— 0	⌣ 5	5
	4				⌒ 5	⌒ $\frac{1}{2}$	$4\frac{1}{2}$
	5				⌣ $\frac{1}{2}$	⌣ 5	$4\frac{1}{2}$
Trial discontinued. Up to standard required by Penn'a R. R.							
$4\frac{1}{8}$ in.	1	1640 lbs.	20 ft.	3 ft.	— 0	⌣ 5	5
	2				⌒ 5	— 0	5
	3				— 0	⌣ $4\frac{3}{4}$	$4\frac{3}{4}$
	4				⌒ $4\frac{3}{4}$	— 0	$4\frac{3}{4}$
	5				— 0	⌣ $4\frac{1}{2}$	$4\frac{1}{2}$
Trial discontinued. Up to standard required by Penn'a R. R.							
$4\frac{1}{8}$ in.	1	1640 lbs.	20 ft.	3 ft.	— 0 in.	⌣ $4\frac{3}{4}$ in.	$4\frac{3}{4}$
	2				⌒ $4\frac{3}{4}$	— 0	$4\frac{3}{4}$
	3				— 0	⌣ $4\frac{1}{2}$	$4\frac{1}{2}$
	4				⌒ $4\frac{1}{2}$	⌒ 1	$3\frac{1}{2}$
	5				⌣ 1	⌣ $4\frac{3}{4}$	$3\frac{3}{4}$
Trial discontinued. Up to standard required by Penn'a R. R.							
$4\frac{1}{8}$ in.	1	1640 lbs.	20 ft.	3 ft.	— 0 in.	⌣ $4\frac{3}{4}$ in.	$4\frac{3}{4}$ in.
	2				⌒ $4\frac{3}{4}$	⌒ $\frac{1}{2}$	$4\frac{1}{4}$
	3				⌣ $\frac{1}{2}$	⌣ 4	$3\frac{1}{2}$
	4				⌒ 4	Broke.	

4½ in.	1	1703 lbs.	20 ft.	3 ft.	— 0 in.	⌣ 6⅛ in.	6⅛ in.
	2				⌒ 6⅛	⌒ ⅛	5⅞
	3				⌣ ⅛	⌣ 6	5½
	4				⌒ 6	Broke.	
4½ in.	1	1703 lbs.	20 ft.	3 ft.	— 0 in.	⌣ 5¼ in.	5¼ in.
	2				⌒ 5¼	Broke.	
4½ in.	1	1703 lbs.	20 ft.	3 ft.	— 0 in.	⌣ 6 in.	6 in.
	2				⌒ 6	— 0	6
	3				— 0	⌣ 6	6
	4				⌒ 6	— 0	6
	5				— 0	⌣ 6	6
Trial discontinued. Up to standard required by Penn'a R. R.							
4½ in.	1	1703 lbs.	20 ft.	3 ft.	— 0 in.	⌣ 6 in.	6 in.
	2				⌒ 6	Broke.	
4½ in.	1	1703 lbs.	20 ft.	3 ft.	— 0 in.	Broke.	
4½ in.	1	1703 lbs.	20 ft.	3 ft.	— 0 in.	⌣6¼ in.	6¼ in.
	2				⌒ 6¼	—0	6¼
	3				— 0	⌣6	6
	4				⌒ 6	Broke.	
4½ in.	1	1703 lbs.	20 ft.	3 ft.	— 0 in.	⌣ 6½ in.	6½ in.
	2				⌒ 6½	— 0	6½
	3				— 0	Broke.	

CELESTIAL CHEMISTRY.—This is a department of science now recognized as a distinct and (by reason of the large amount and wonderful character of the knowledge which it already includes) an important branch of modern research; and we propose at the present time to group together under the above general heading, a number of interesting results obtained within the last year, and in some cases briefly noted by us before at the time of their first announcement, but of which full accounts have now for the first time reached us, chiefly in a large package of the Proceedings of the Royal Society, which has just arrived. Taking these in the order of their novelty, as far as may be consistent with a consistent classification of subjects, we will begin with—

The Spectrum of Comet II of 1868, and its coincidence with that of ignited carbon vapor. In the Proceedings of the Royal Society, p. 481, Mr. Wm. Huggins gives an account of his observations and experiments with regard to this object. The comet as

seen in the telescope has the appearance as shown in the accompanying cut, Fig. 1, consisting of a nearly circular coma, becoming

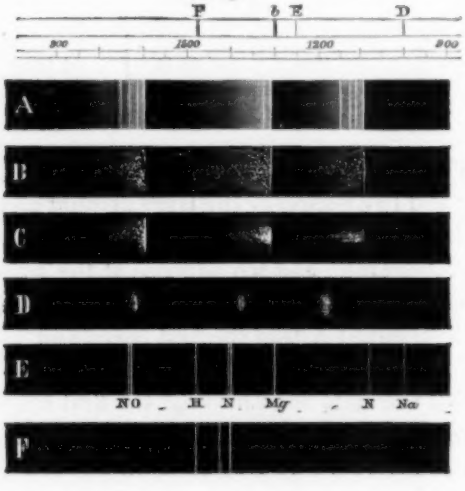
Fig. 1.

suddenly bright towards the centre, which consisted of a nearly round spot of light. The tail, of a hirsute character, extended nearly one degree (about twice the apparent diameter of the moon).

When the light from the circular head was examined with a spectroscope having two prisms of 60°, three bright bands were observed such as are shown in C of Fig. 2.

The two of these bands towards the left faded off towards that side by a regular gradation, but the other did not show this character. These bands showed no indication of divisibility into lines, nor did any of them shade off towards the left or violet end of the spectrum. On comparing these bands with those of other spectra, it was found that they coincided exactly with those given by ignited carbon vapor under certain conditions. Thus in 1864, Mr. Huggins had observed that when the spark from an induction coil with leyden jars in circuit, was taken in a current of olefiant gas, the spectrum shown in B, Fig. 2 was obtained. This coincided with the carbon spectrum obtained under other conditions, except that no separate, strong lines were to be distinguished, and in place of the shading composed of fine lines, found in what might be called the normal carbon spectrum, (see Fig. 2,

Fig. 2.



A, showing the spectrum of the spark in olive oil,) an irresolvable nebulous light was here given. It should be here remarked, that in the actual experiment, the lines due to the other bodies, such as Hydrogen and Platinum, were also seen, but these being well known, were readily eliminated. The drawing shows the lines due to carbon alone. On the 23d of June, 1868, the spectrum of the comet was compared directly with the spectrum of the spark in olefiant gas, and the identity of position and character between the bands was then very manifest, as appears in Fig. 2, B and C.

We are thus led to the strange conclusion that this comet is composed of luminous carbon vapor. Now we know, that to vaporize carbon, as we have it within the range of experiment, requires heat of great intensity and peculiar conditions of chemical reaction, as in the case of a jet of olefiant gas supplied with air, which gives a a greenish flame, resolvable into the carbon spectrum.* In this connection we would refer to the paper on "Sources of Light in luminous Flames," at page 330 of this number.

How any such conditions of temperature as those developed by the induction spark which produces carbon vapor directly, or those of the mixed air and gas flame where it is indirectly developed, can subsist in a comet, we are utterly at a loss to imagine. This mystery, however, does not stand alone, for in the case of the gaseous nebulæ we have a like example of self-luminous gas maintaining its luminosity with a similar absence of adequate cause. It would appear possible that some action related to phosphorescence or to fluorescence might be concerned in this result, but such a conjecture is unsupported by any evidence of similarity between the spectra of such bodies and of these celestial objects. We can then simply regard this comet as a vast globe of glowing carbon vapor, having a density approaching that of our atmosphere, (for it is at atmospheric pressure that the spectrum B, Fig. 2, is given,) maintained in its luminous state by forces and conditions of which we have absolutely no knowledge.

In connection with this subject, Mr. Huggins suggests that the enormous velocity with which the tails of comets are repelled from the sun, may be connected with a previous dilation of substance by means of which the particles have been separated beyond the range of cohesive attraction.

* We have just observed that the same effect is obtained with a Bunsen burner, supplied with common gas, by observing the lower part of the flame.

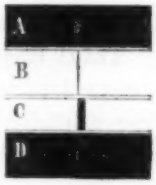
Spectrum of Brossen's Comet.—A notice of this spectrum at the time of its first observation was published in this *Journal*, Vol. LVI, p. 155, but we now give, in D of Fig. 2, a drawing of its bands, which may be compared with the bright lines of certain elements given with the induction spark (see E), and with the lines of the nebulæ, such as 37 H, IV Draconis, 1 H, IV Aquarius, the great nebulæ in Orion, and several others. It is evident at a glance that this spectrum differs essentially from that of the other comet, but no conclusion seems as yet to have been reached as to its probable constitution. The division of the middle band into two bright lines is a curious feature, worthy of note.

Physical Constitution of the Sun, as revealed by the Spectroscope.—In a preliminary note concerning observations and experiments in this direction, which they have now in progress, Prof. Frankland and Mr. Lockyer briefly notice several interesting points. Reference is first made to the paper by Mr. Lockyer, of which we republished the abstract in our last number (p. 266), and in which attention is directed to the fact that, (1) there is a continuous gaseous envelope about the sun, called the "chromosphere," and that the H β line, (corresponding to Fraunhofer's F,) in the spectrum of this envelope, widens out as it approaches the body of the sun, so as to have a lance-head shape. (2). Ordinarily in a luminous prominence, the line H β is nearly of the same width as H α, (corresponding with C), but that sometimes H β widens out so as to present a bulbous appearance above the chromosphere. See Fig. 5, plate facing p. 268, last number. (3). The lines H α and H β extend over the photosphere and reverse the corresponding Fraunhofer lines. (4). There is no Fraunhofer line corresponding to the one near D in the spectrum of the chromosphere. (5). There are many *bright* lines visible in the light of the photosphere near the edge. (6). A new line occasionally appears in the spectrum of the chromosphere.

In following out the investigations indicated by these observations, they have, in the first place, been unable to develop in the spectrum of hydrogen any line corresponding with that near D, in the spectrum of the chromosphere. Secondly, they have found that the expansion of the lines in the spectrum of hydrogen, already described by Plucker and Hittorf, is due to the *pressure* in the gaseous medium, and not at all to temperature *per se*. Thus, in the researches of these German savants, quoted by us on p. 272 of our last number, it was mentioned that when the spark length was

increased, the broadening of the lines occurred. Now, we know that this increase of spark length implies an increase of temperature under the conditions involved in these experiments, but it also must increase (in the closed tubes which were employed to contain the gases) the tension or pressure on these expansible fluids. The previous experiments, therefore, did not indicate to which of these influences the expansion of the lines was due. This has, however, been accomplished by Messrs. Lockyer and Frankland, and though we are not yet informed as to the methods pursued, these are not difficult to imagine. In this connection it is curious to note that the spectrum of the star Sirius, observed by Mr. Huggins with the instrument of great dispersive power, prepared for the measurements of advance or retreat of stars, with reference to the earth (noticed at page 155 of our last Volume), shows an expanded hydrogen band, F, much broader than that found in the solar spectrum, and corresponding in fact with that obtained from the spark in hydrogen at a pressure approaching that of the atmosphere. These relations are illustrated in Fig. 3, where A represents the F line, as given at this partial atmospheric pressure, D the same in an exhausted tube, B the F line of absorption in the solar spectrum, and C that in the spectrum of Sirius.

Fig. 3.



Now, it has long been surmised that the mass of Sirius is far greater than that of the sun, (393 times as great, according to the usual estimate,) and thus a greater density of the heated hydrogen at its surface, would be exactly what we might expect to find.

In our next number (want of space now precludes us), we will give a full account of the very curious investigations as to the proper motion of the stars above noticed, and of which we have now at hand full details.

To return to the "Note" of Messrs. Frankland and Lockyer; they further remark, that their experiments have shown the hydrogen gas contained in the solar prominences to be in a condition of extreme tenuity, the bulbous appearance of the F line indicating local currents or intensities of temperature.

Thirdly, from the absence of any black line in the solar spectrum corresponding with the bright one near D given by the chromosphere, implies, that if this is a hydrogen line, the absorption of the atmosphere is unable to reverse it. In explanation of this, it must

be remembered that the light which gives us the spectrum of the chromosphere comes from a stratum of great thickness, measured in the direction of the line of sight (nearly 200,000 miles), while the thickness of the chromospheric shell itself, which alone is effective in absorbing and reversing the rays, is small in comparison (5000 miles). Thus the one might produce a luminous line of appreciable force, while the other would not absorb enough light to develop a sensibly dark space in the solar spectrum.

Fourthly, it would appear that the photosphere is no longer of necessity to be considered as composed of incandescent solid or liquid matter, but may be of gaseous consistency, this being consistent with its continuous spectrum. (This follows, in fact, from the observations of Plucker).

Fifthly, the remarkable fact is next stated in this preliminary note that: *Bright lines have been discovered in the spectrum of the photosphere*, in the vicinity of the solar edge. The evidence of such lines had been surmised by Prof. Stoney, and in a communication to the Royal Society, May 15, 1867, he alludes to the probable position of several.

Tidal Rains.—In the *Journal* for November, 1868 (p. 304), we referred to Mr. P. E. Chase's investigations of tidal influences on the fall of rain. In the present number, we give some of his curves, which seem to furnish conclusive evidence of the following tendencies:—

1. A diminution, both in the frequency and in the amount of rain, near the times of new and full moon, with an increase in the subsequent octants.

2. A resemblance between the lunar-daily and the solar-hourly barometric changes. "There are, however, only two normal barometric maxima and two minima during the solar day, while there are three of each during the lunar month. When the moon's upper culmination occurs at night, she intensifies for the whole day, the barometric tendency of the corresponding solar hour; when it occurs by day, this intensification is accompanied and controlled by a marked priming and lagging which introduce an additional inflection into the lunar curve, the normal barometric tendencies being accelerated before, and retarded, after new moon."

3. Triple periods of increase and diminution in the lunar-hourly and the lunar daily rains, nearly corresponding in period and opposed in direction to those of the barometer.

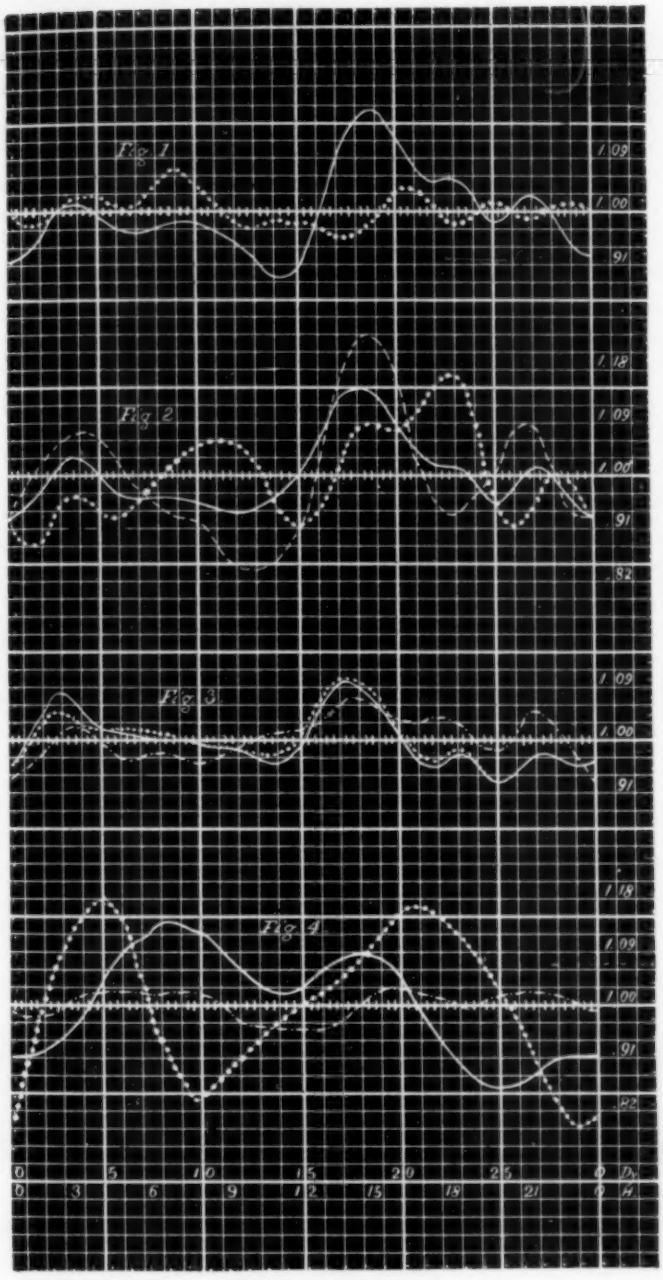


Fig. 1
Fig. 2
Fig. 3
Fig. 4
1.09
1.00
.91
1.18
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0 5 10 15 20 25 0 Dr.
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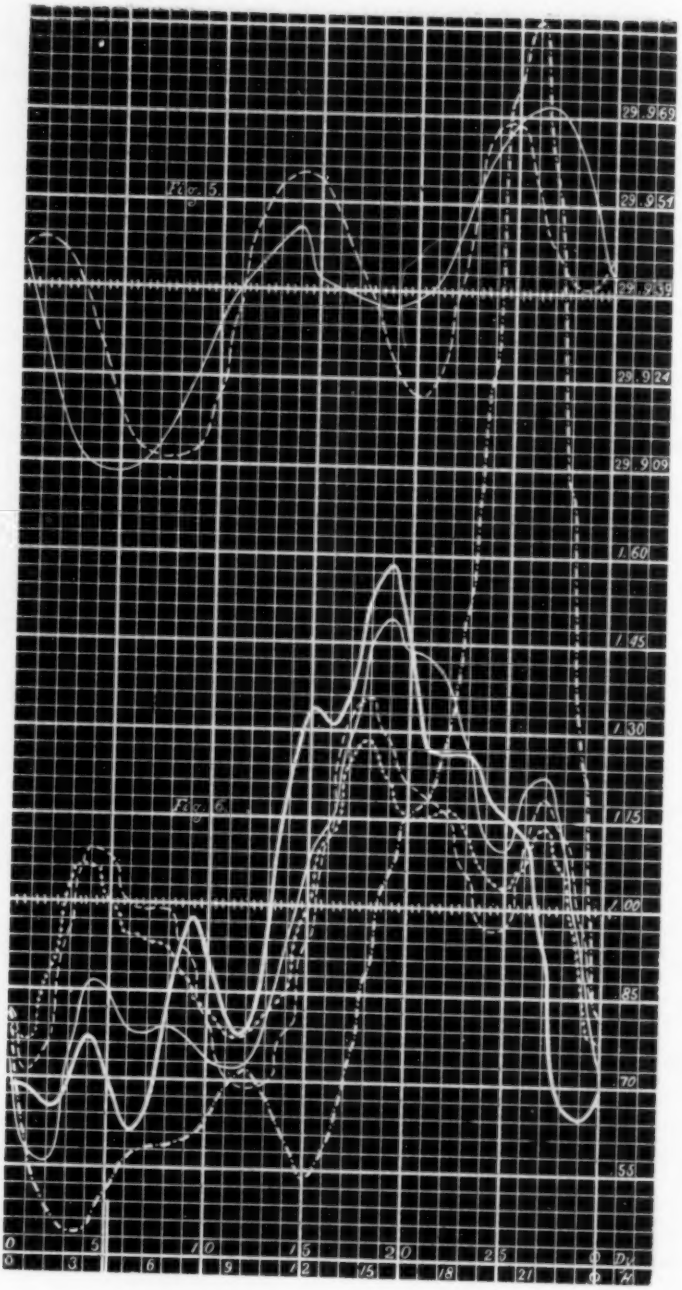
29.969
29.954
29.939
29.924
29.909
1.60
1.45
1.30
1.15
1.00
.85
.70
.55
Fig. 5
Fig. 6
0
5
10
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25
0
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12
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Ø
M

4. Heavy rains after full moon, "or during that portion of the lunar month when the moon's action is intensified by a falling barometric and increasing condensation of atmospheric vapor."

At the recent meeting of the National Academy of Sciences, in Washington, Mr. Chase exhibited some of the curves which he had computed for the Coast Survey. They illustrated several interesting facts, among which were the following:—

1. Opposite tendencies at the equinoxes and solstices.
2. Opposite tendencies at Greenwich and Philadelphia, or on opposite sides of the ocean.
3. The influence of mountain ranges on the times, as well as on the amounts of precipitation.
4. Planetary influence.
5. Cycles of dry and rainy years, dependent on the relative positions of the moon and principal planets.

EXPLANATION OF FIGURES.

The notched horizontal line, in each figure, represents the mean daily or hourly value; each vertical space represents a deviation of ·03 of the mean value, in the rain curves, or of ·003 of an inch in the barometric curves; each horizontal space represents a day in the abscissas of the monthly curves, or forty-eight minutes in the abscissas of the daily curves.

Fig. 1. Total lunar-daily rainfall.
Philadelphia, 1825–44; ——— continuous line.
Surrey, 1825–44; ..... dotted line.

Fig. 2. Total lunar-daily rainfall at Philadelphia.
1825–44; ..... dotted line.
1845–64; - - - - - - - - - broken line.
1825–68; ——— continuous line.

Fig. 3. Lunar daily rains at Philadelphia.
No. of storms exceeding ·25 in.; ..... dotted line.
Average fall; - - - - - - - - - - - - - - - - - broken line.
Amount of rain in moderate storms; ——— continuous line.

Fig. 4. Total hourly rainfall.
Greenwich, solar hourly; ..... dotted line.
Philadelphia; " " ——— continuous line.
" lunar hourly; - - - - - - - - - broken line.

Fig. 5. Barometric fluctuations at Philadelphia.
Solar hourly; ———————— continuous line.
Lunar daily; - - - - - - - - - - - - broken line.

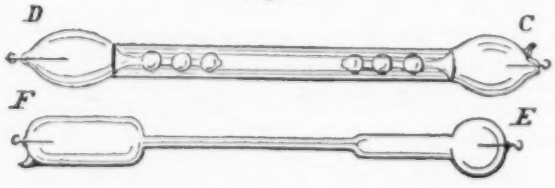
Fig. 6. Heavy rains at Philadelphia.
Lunar daily amounts, in storms of 1 inch or more; dotted line.
Do. do. do. 1·5 in. or more; broken line.
Do. do. do. 2 in. or more; light line.
Do. do. do. 2·5 in. or more; heavy line.
Solar-hourly, exceeding 25 in. per hour; dotted broken line.

Invisible Light.—Many years since, a photograph was made at Berlin of the well known bronze statue of the Amazon, and it was observed that in the negative a black streak occurred at the tip of the lance (held by the figure in an almost vertical position), while two other analogous marks appeared in other locations. This picture was sent to Professor Dove, whose investigations in connection with light are widely known, and he came to the conclusion that these markings might be due to electrical discharges going on from prominent points of the figure at the time the picture was taken, and which, though invisible to an observer, would nevertheless, by reason of the high actinic power of electric light, produce an impression on the photographic plate.

This conjecture was fully confirmed by Professor O. A. Rood, of Columbia College, N. Y., who, in a series of ingenious experiments, proved, that electric discharges entirely invisible to the observer in the presence of daylight, might nevertheless produce images of themselves in a picture of the adjacent objects taken at the same time—the photographic plate being relatively more sensitive to these impressions than the human eye.

Among other means used to demonstrate this fact, a number of Geissler tubes were employed. These, as our readers know, are glass tubes of various shapes, such as the accompanying cuts illus-

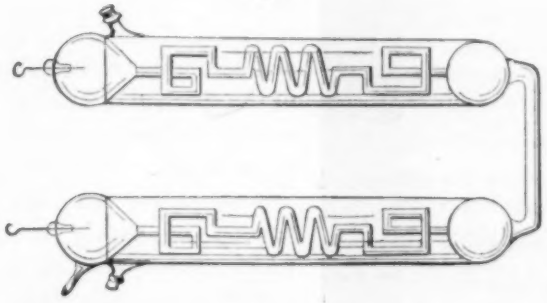
Fig. 1.



trate, very perfectly, though not absolutely exhausted, and sealed up, but with platinum wires secured in their extremities. When

the electric discharge from an Induction Coil was made to pass through these, it developed a delicate purple and blue, or sometimes pink light, which, though visible in the dark, was not perceptible in daylight. A photograph, however, of these tubes, taken in daylight, showed as well developed an image of the interior light, or

Fig. 2.



discharge, as of the tube itself. If, however, a "fluorescent"* medium were placed around the tube carrying the discharge (as when the jackets in such a tube as Fig. 2 were filled with a solution of quinine), a bright light was visible, but no impression was produced upon the sensitive plate. The practically invisible actinic rays were here made visible, but lost in photographic force what they gained in optical power. We have thus far cited examples of invisible light, which, though properly indicated by the title used, were still not perfect in their invisible character, since, under a new state of the surrounding conditions they might cease to possess the characteristic of invisibility.

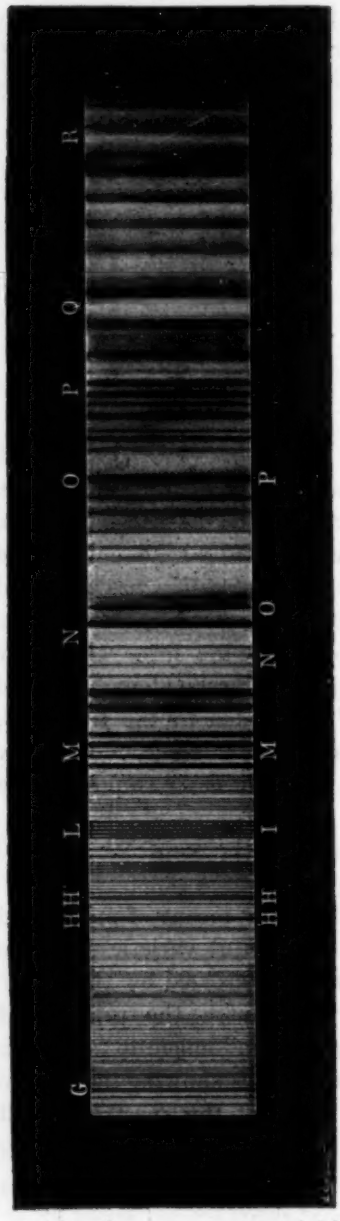
We have, however, other illustrations of the same paradoxical condition which are free from this objection.

If a spectrum is produced with sunlight, by means of prisms and lenses of quartz, we shall see all the beautiful varieties of pure blending colors known as the rainbow tints, crossed by the black lines, named after Fraunhofer, exactly as with the prisms and lenses of glass, but if we allow this "spectrum" image to fall upon a collodion plate, the picture produced (besides showing the visible bands in the blue, indigo and violet parts of the spectrum), will give an equally distinct drawing of similar bands or lines, reaching

* Fluorescent bodies are those which have the power of absorbing very rapid vibrations, and re-emitting them in a condition of slower motion; taking up, for example, actinic and invisible rays and emitting them as luminous ones.

to a great distance beyond, what, to the eye, is the end of the luminous stripe. Fig. 3 shows the important part of such a picture. It is copied from an original photograph mentioned in this Journal, Vol. LVI, p. 303, made by Prof. Muller. The line G is one of those known and named by Fraunhofer, and occurring in the indigo portion of the spectrum; H and H′ were also named by him, and occupy the extreme violet. Near these the visible spectrum terminates, and all beyond is purely actinic force, which cannot be appreciated by the eye directly, under any conditions. This part of the picture (all beyond H′, in other words,) may be regarded as a photograph not of *light*, but of *darkness*. Not, however, of *any* or *all* darkness, but of a certain kind of darkness only. The letters on the cut above the spectrum are those assigned as names to the important groups of lines by M. Mascart, who has written a work on the extra-violet spectrum; while the letters below are those assigned by M. Becquerel, to whom this department of science owes very much. This portion of the spectrum may be made appreciable to the eye *indirectly*, if it is allowed to fall on a screen made of some fluorescent substance, when, as in the case of the Geissler tubes, before mentioned, the motions or vibrations too rapid to affect the eye, will be so diminished in their rate as to develop some appreciable color varying with the fluorescent substance employed.

Fig. 3.



Civil and Mechanical Engineering.

GAUGING THE FLOW OF RIVERS, &C.

By Clemens Herschel, C. E.

On a means of measuring the quantity of water flowing in rivers, canals, etc., by the method of first measuring the *velocity* of this flow at a number of *points* in the cross section of the stream.

In the Lowell Hydraulic Experiments of James B. Francis, we have the record of a series of experiments on the flow of water over weirs of a certain construction, such flow during a certain time being gauged by direct measurement after entering a receiving basin of known and regular dimensions.

In the second edition of the same work, we find, further, the record of a series of experiments on gauging the flow of water in rectangular canals or flumes, by means of loaded tubes or poles, which measurements were corrected by gauging the same body of water as it passed over a weir, by the previously established reliable method or formula. We can, therefore, not only take this work to be a *standard* authority on these two methods of gauging the flow of water, but we can also, without I think exaggerating the value of the book, call it a standard authority on these two subjects for all time.

There arise, however, many cases in which neither of the above two methods are practically or conveniently applicable, and it is to meet these, that the method indicated in the heading, is of great service. Such cases are, in the first place, all natural streams, large and small rivers, brooks, etc., that have no dams across them at the place of investigation, also rough canals, and even well constructed rectangular flumes, provided they are not long enough to admit of a good measurement with tubes, and generally, when as so frequently happens, a weir is inapplicable on account of the back-water it causes, and a long and properly situated rectangular flume cannot be put in, either on account of want of room, or of time, or because the same would cost more than the parties interested are willing to expend.

In these cases, if we find the cross section of the stream on any given line, divide it into any chosen number of parts, measure the

velocity of the current at the centre of these parts, and multiply their area by these measured velocities, we get, of course, in the sum of these products, the total quantity flowing through the whole cross section.

Practically considered, the accuracy of such a measurement will depend *first*, on making the partial areas sufficiently small, which implies the measuring of the velocity in a sufficiently great number of points; *secondly*, on the accuracy of the instrument used to observe the velocity of the current, and on the accuracy with which it is used, and *thirdly*, and finally, on introducing a coefficient, to be determined by experiment—just as the weir and tube formulæ were found and corrected by experiments—because we cannot measure the velocity of those water lines which are in immediate contact with the periphery, as will appear presently. It may be well to state at once that this coefficient is small, and I doubt if on trial, provided the experiments are conducted with the accuracy of those described in the Lowell Hydraulic Experiments, it will be found to affect the total result in the case of canals as much or more than 3 per cent., and perhaps no more in the gauging of rivers of any size. Hitherto, this coefficient has never been considered at all. The best instrument to measure the velocity of the current at any point, has generally been thought to be the so-called Woltman wheel, or tachometer (Woltmanscher Flügel Ger., moulinet de Woltman Fr.) descriptions of which may be found in Weisbach's Mech., Vol. II., Bauernfeind Vermessungs kunde, Annales des P. & C., Nov. and Dec. 1847, etc.

It is perhaps unnecessary to repeat these here; any interested reader of this, will look them up, provided the subject is not already familiar to him, and those that do not care to do this, will have to get along the best they can.

Let us assume, therefore, that this little instrument, invented as far back as 1780 or '90, is well known to us all, and proceed.

To simplify matters, and at the same time to introduce in the English language an easy sounding name for the instrument, we will call every instrument that measures the velocity of a current, by the number of revolutions imparted by this current in a certain time, to a body revolving on an axis of whatever shape, a "moulinet" (French name for them, signifying little mill), and a gauging made by using such an instrument, a "moulinet measurement."

There have been two chief objections to the use of Woltman's

moulinet, as taught in the various text books and articles; one was the trouble of being obliged to lift the instrument and the rod to which it was attached, out of the water at every reading; the other was the trouble of first determining by experiment what that reading meant, when expressed in velocity of feet per second. There may be added the danger of injury to the moulinet, but as will be shown, this danger can be done away with for the greater part, by a more rational arrangement of the whole apparatus.

As regards now the first named evil, a very serious one, as it wastes at least half the available time, and also exposes the moulinet to great danger of injury, it is a pleasure to be able to record, that it has been entirely and very neatly eradicated by an invention of Mr. D. Farrand Henry, Assistant of the U. S. Lake Survey, which has been employed in the gauging of the rivers connecting (or leading to the sea), the Great Lakes, under the charge of General W. F. Raynolds, U. S. Eng., and stood the test of service. It consists, in the abstract, of the idea of connecting the revolving float or propeller wheel, etc., by means of an electric circuit with a register above water, so that each revolution is recorded as it is made.

The Figs. 1—5, show two of the telegraphic moulinets employed, and the manner of their use. Fig. 1 shows the cups and wheel of a Robinson anemometer,* as they were arranged and used as a telegraphic moulinet.

On the axis is a small arm, which comes in contact with a fine silver wire at every revolution. This wire, in order to give it more elasticity, is formed into a spiral, then is insulated from the frame, and finally connected with one of the battery wires; the other is connected with the frame in which the wheel is hung. This frame

* This anemometer takes its name from that of its inventor, Dr. Robinson, of Armaugh, Scotland. Its construction is based upon Dubuat's observations, that the resistance of a sphere floating in water is to that of its great circle as 0·35 : 1. (See Principes d' hydraulique Vol. II., p. 263.) Beaufoy found this relation to be 0·342, and hence Robinson supposes (without just cause it seems to me), that a four armed wheel having four hemispherical cups at its periphery (see Figs. 1 and 5), would revolve with a velocity of revolution, at the centres of the cups, equal to one-third of that of the current of air to which it was exposed. The anemometer wheels used, gave very good results in *water*, inasmuch as they registered velocities as low as 0·35 feet per second, but the "coefficient" of their revolution varied with the velocity observed, being more and less than one-third, as may be seen hereafter. The anemometer wheels here spoken of were made by James Green, of New York.

is supported in a second frame, behind which latter is fastened a vane to keep the axis in the line of the current, as shown in Fig. 5.*

Fig. 2 shows another kind of moulinet, in which the wheel is a kind of propeller. In this one, an eccentric is placed upon the hub of the propeller, and a little roller at the end of the ivory lever is pressed against it by means of an adjustable spring. On the under side of this lever, projects a small piece of platinum wire, and this wire comes in contact with a platinum plate just below it, whenever the eccentric is at its minimum. This platinum wire is connected by means of an insulated wire, with one pole of the battery, and the axis of the propeller with the other. This propeller also is hung in two frames, and is furnished with a vane similar to the first mentioned.†

Figs. 3 and 4 show the register in front and rear elevation. It is a very simple contrivance, and consists of a common telegraphic sounder, which moves the first cog wheel forward one cog, at every movement of the lever, by means of a pawl and ratchet. This wheel gears with a ten-leaved pinion into the second hundred toothed cog wheel, and we have thus a register up to 1000 movements of the sounder. Two hands serve to indicate the number of these movements on two dials. The mode of operation is shown in Fig. 5, (not drawn to scale.) A boat in this case, 15—16 feet long, and about three feet beam, is furnished with an anchor, for a current of about six feet per second, to weigh forty pounds, and a dead weight of the same weight. At the fluke end of the anchor-stock, is fastened a line about 200 feet long, and another line is made fast to the head ring. The boat is now rowed out into the stream to a point about 200 feet above the place of observation, the anchor is thrown out and the boat is allowed to drop down with the current until the line attached to the fluke-end has nearly run out. This line is now made fast to the forward ring of the dead weight, and a second line and a strong copper wire are made fast to the upper ring. The dead weight is now lowered, and the boat is at the same time permitted to drop down stream until finally, when the weight is at the bottom, it is vertically under the stern of the boat. The forward line is now made fast, and the line fastened to the upper ring of the

* The drawings are not to scale, but the circumference of the circle described by the centres of the cups is given = 3·52 feet, whence and from the following tables, the velocity of revolution, etc., may be calculated.

† Of course any other kind of wheel, such as that of Woltman's, for example, may be fitted in a similar manner to register by telegraph.

Fig. 3.

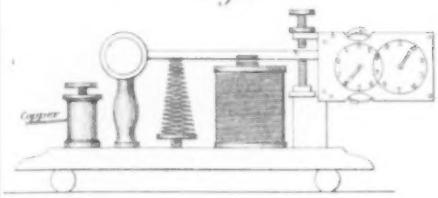


Fig. 1.

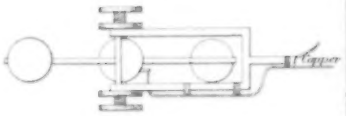


Fig. 4.

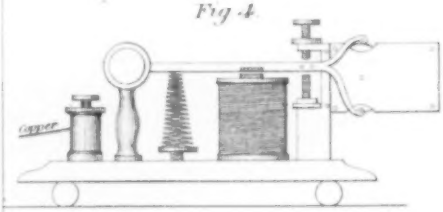


Fig. 2.

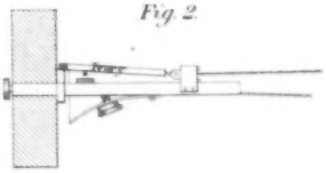
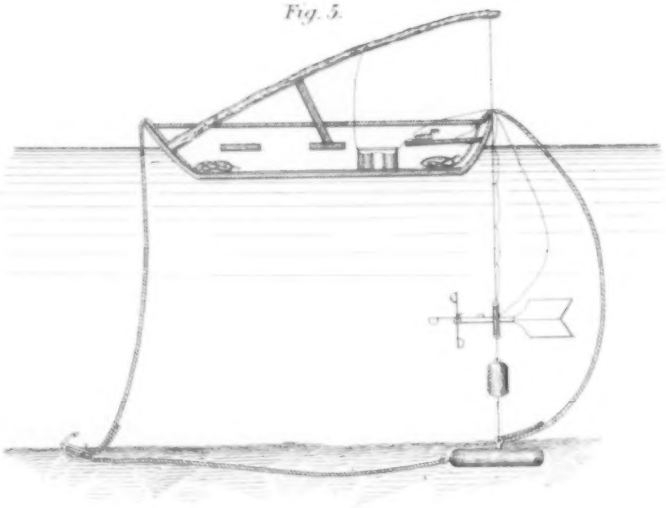


Fig. 5.



weight is let out slack, so as to be out of the way, and is then made fast astern.

As may be seen in the figure, there is a spring-pole over the boat. The after end of this pole is bent down, and the copper-standing wire is fastened to it. This arrangement serves to keep the standing wire taut, and to take up small wave movements of the boat. The outer frame of the moulinet has a swivel ring at the top and bottom; a weight is attached to the lower, and a measured cord, having small snap hooks every five feet, to the upper one. These hooks are snapped around the standing wire, thus keeping the measured cord as straight as may be, instead of being bowed out with the current.

On one side of this same frame are two eyes, through which is placed the standing wire, and along which wire, the whole apparatus may now be raised or lowered without disturbing the electric current in its passage from the frame through the eyes and the standing wire to the battery. The moulinet is now lowered to a depth of say five feet, and the ends of the standing and insulated wires are connected with the battery and register in the usual manner. A common electric switch serves to put the register in and out of connection, and this enables us to observe the number of revolutions made by the moulinet wheel in any desired length of time. The moulinet may now be lowered to any other desired depth by means of the measured cord, the position of the boat being determined at each place of observation by measurements from the shore. When all desired moulinet observations have been made at one place, the instrument is hauled up and detached from the standing wire, the upper end of the same is let loose, the dead weight is hauled up on the upper ring and line, and the anchor may then be easily raised, even from a clay bottom by means of the connecting line, which pulls directly on the flukes. The above account is reproduced from data furnished by Mr. Henry, and although the full results of his observations are not yet ready for publication, the following notes may be of interest. Observations were taken on a fixed line every 100 feet width, and every five feet depth. It was at once found—and a Woltman moulinet when running near the surface shows the same thing—that the water had no *uniform* motion, but *pulsated*, so to speak. These pulsations again were not regular, but had a common maximum every minute and a half, and a still greater maximum or a minimum every five or ten minutes. These pulsations were weakest in the main current, and largest near

the shores and bottom. A table of the discharge of the five rivers that connect and bring to the ocean the waters of the Great Lakes, was published in the number of the *Journal Franklin Institute* for January, 1869.

Careful experiments have also been made to compare the velocities as indicated by submerged floats—such as were used by Generals Humphreys and Abbott—with those given by the telegraphic moulinet, from which it appears that the former show a velocity slightly in excess of the true one, caused by the upper float dragging the submerged one with it.

It will be remembered that in all moulinets, the so-called "coefficient" of the instrument (=the velocity of the current in feet per second divided by the number of turns per second), has played an important part, and although, as will presently appear, I believe this troublesome member may be entirely abolished, yet it is only just to insert here, Mr. Henry's studies on the same, the more so as they were conducted with great care and accuracy. Mr. Henry also claims to have discovered that the coefficient of *all moulinets* varies with the velocity measured and according to a given ellipse, or a part of the same, in which the abscissæ represent the velocities, and the ordinates the coefficients. The coefficients of the two moulinets here drawn, were found by fastening them under the middle of a boat, which was then pulled by a rope with various velocities through a lake, and over a course of about 500 feet.

The following Table I. gives the results of these experiments, and it is to be noted that the given figures are the mean values of 20—30 observations in case of the slow velocities, and 8—15 observations for the others.

These coefficients appear to follow some law of increase, and Mr. Henry finds that they approximate nearest to forming the ordinates of a quarter ellipse, whose semi-major axis, representing the velocity in feet per second, equals 4·1, and whose semi-minor axis is equal to 1·72 on the same scale. Now, in order that we may read the coefficients of various moulinets, from this one curve, the same must be read off, each according to its proper scale, or arithmetically, the ordinates of the curve must be first multiplied for each moulinet with a constant, empirical and calculated number, and besides this, in certain cases, the axis of Y must be moved along the axis of X, by a certain constant, so as to bring the two curves to cover each

other. Mr. Henry has completed this investigation for four different moulinets, with the following results.

In Table II., are given the values for the two above described moulinets, and for a third one described in Morin's Hydraulique, p. 100, et sequ., and also mentioned in Weisbach. In the table it is called the Lapointe moulinet, after its inventor. Table III. gives the values for a Woltman moulinet described in the Annales des Ponts et Chausseès, November and December, 1847, by Baumgarten. The number of revolutions which form the bases of the coefficients, were found for the cup, propeller, and Woltman moulinets by direct experiment; for the Lapointe moulinet they were calculated, but likewise from experimental data which may be found as above indicated.

TABLE I.

Velocity in ft. per sec.	Cup-Moulinet.		Propeller-Moulinet.	
	Revolutions per sec.	Coefficient.	Revolutions per sec.	Coefficient.
0·3	0·0000	.....	.....	.....
0·5	0·0391	12·778	.....	.....
1·0	0·0900	11·123	0·558	1·757
1·5	0·1461	10·268	0·872	1·680
2 0	0·2057	9·722	1·213	1·629
2·5	0·2715	9·208	1·514	1·584
3·0	0·3375	8·888	1·897	1·562
3·5	0·4050	8·638	2·229	1·550
4 0	0·4657	8·589	2·635	1·544
4·5	0 5292	8·504	2 947	1·542

A few still greater velocities are given for the Lapointe moulinet, and the corresponding coefficients plot on both sides of a tangent to the ellipse, that is, a perpendicular to the minor axis.

This table is not so satisfactory as Table II., which is perhaps accounted for by its being based on a smaller number and less complete series of observations than Table II.

In looking at the subject from a purely practical point of view, the question naturally arises, what value these and previous investigations with respect to the coefficient of a moulinet, really have.

TABLE II.

Velocity in ft. per second.	Ordinates of Ellipse.	Coeff. of Cup Moulinet.			Coeff. of Prop. Moulinet.			Coeff. of Lapointe Moulinet.		
		Observed.	Computed.	Diff.	Observed.	Computed.	Diff.	Observed.	Computed.	Diff.
0·3	0·000	.....	14·573	.....	.....	.....	.....	.....	.....	.....
0 5	0·431	12·778	12·704	+ 0·074	.....	.....	.....	.....	.....	.....
0 65	0·694	.....	.....	.....	.....	1·880	.....	.....	.....	.....
1·	0·961	11·123	11·190	— 0·067	1·757	1·744	+ 0·013	.....	.....	.....
1·5	1·214	10·268	10 300	— 0·032	1·680	1·673	+ 0·007	.....	0·650	.....
2·	1·395	9·722	9·662	+ 0·060	1·639	1·627	+ 0·002	0·571	0·572	— 0·001
2·5	1·524	9·208	9·208	0·000	1·584	1·595	— 0·011	0·546	0·540	+ 0 006
3·	1·617	8·888	8·881	+ 0 007	1·562	1·572	— 0·010	0·519	0·519	0·000
3·5	1·678	8·638	8·686	— 0·048	1·550	1·556	— 0·006	0·507	0·502	+ 0·005
4·	1·712	8·589	8·546	+ 0·043	1·544	1·545	— 0·001	0·496	0·493	+ 0·003
4·5	1·720	8·504	8·518	— 0·014	1·542	1·540	+ 0·002	0·486	0·485	+ 0·001
5·	.....	.....	.....	.....	.....	.....	.....	0·477	0·480	— 0·003
5·5	.....	.....	.....	.....	.....	.....	.....	0·474	0·478	— 0·004
		Sum.....		+ 0·015			— 0·004			+ 0·007
		Mean....		+ 0·0017			— 0·0005			+ 0·0009

Regarding the comportment of this coefficient, there is a difference of opinion. Hagen, one of the most eminent of German Hydraulic Engineers (see Wasserbau, II., 1 p. 257), and Bauernfeind (see Vermessungskunde p. 377, et sequ.) and Morin, hold that the number of revolutions per second of two of the above moulinets, are directly proportional to the velocity of the current, that is to say, each moulinet has a certain peculiar coefficient, which may be used in measuring all velocities, and consequently ought really to be found and indelibly engraved on the instrument by its maker.

Of an opposite opinion, speaking of the same two moulinets and two others, are Baumgarten (Annales des P. & C. Nov. et Dec. 1847), Weisbach (Vol. II.) and Mr. Henry, who claim to have found that this coefficient varies with the velocity measured.

TABLE III.

Velocity in ft. per sec.	Ordinate of Ellipse.	Coefficient of Woltman Moulinet (according to Baumgarten).			
		Observed.	Selected and mean.	Computed.	Difference.
1·05	.....	.....	.....	1·525	.....
1·063	.....	1·606	.....	.....	.....
1·138	0·352	1·493	1·493	1·477	— 0·016
1·325 1·402	0·656	1·405 1·445	1·425	1·435	+ 0·010
1·617	0·798	1·368	1·368	1·416	+ 0·048
2 139 2 188	1 177	1·364 1·324	1·344	1·364	+ 0·020
2·575	1·366	1·306	1·306	1·343	+ 0·037
2·835	1·421	1·342	1·342	1·331	— 0·011
3 845	1·631	1.291	1·291	1·302	+ 0·011
6·263	1·720	1·305	1·305	1·290	— 0·015
		Sum.....			+ 0·084
		Mean.....			+ 0·0105

If we now, as has usually been done, express the relation of the number of revolutions to the current velocity by several coefficients and a formula, then the mere thought that it is necessary to enter into a long calculation to find the velocity corresponding to each reading of the instrument, induces one to abstain from the use of such an apparatus, and this is perhaps to a great extent the very reason why the moulinet has been so little used in comparison to floats and other less accurate objects for measuring velocities.

What is wanted, is the means of knowing at once what each reading of the moulinet signifies when expressed in feet per second, and

the setting up of a curve, or what is the same thing, of a law for the relation between the number of *revolutions* and the velocity or between the *coefficients* and the velocity, is an outside matter and a *departure* from the object sought.

Again, should any one be called on, say, to testify in court respecting any measurements in which a moulinet had been used, he could not, after all, rely on computed values; but should he have made direct experiments, at different velocities, like those of Mr. Henry with the identical instrument, he could then say with certainty just what velocities had been observed.

If we plot the number of revolutions of the moulinet in one minute* as ordinates, and the corresponding velocities expressed in feet per second as abscissæ on good diagram paper (paper divided off into squares), and choose a proper scale, then the velocities corresponding to every five whole number of revolutions per minute, may be read off and tabulated at once, without committing an error $> 0{\cdot}05$ feet per second, the more so since the curve thereby formed is a very regular one, and coincides almost exactly with the given points. Now, interline the differences and equalize them, and we will get finally a table of velocities in feet (to three places of decimals) per second corresponding to every whole number of revolutions of the moulinet in one minute and ranging, to take the example of a table for a Woltman moulinet, I had occasion to make last Fall, from 10—320. A Woltman or any other moulinet is incomplete without such a table, and is then regarded as an object of curiosity, more frequently, than as an instrument of service.

It is now an easy matter always to let the moulinet register when taking the observations, a minute, an aliquot part, or an even multiple of a minute, and then the corresponding velocity may be at once found from the table. If the moulinet is allowed to register twice in succession for a half minute in one place, then the sum of these two readings gives the mean of two minute readings, and the table gives us the observed velocity. We need now only divide the cross section of the river or canal into parts that contain a whole and *convenient* number of square feet, measure the velocity in the centre of each of these, and then a series of *mental* multiplications and *one* addition suffices to give the total quantity in cubic feet per second.

As above mentioned, the smaller the partial areas, the more accurate will be the measurement. A variation in the observed number

* Why a *minute* is chosen, will appear presently.

of revolutions is caused quite as likely by the want of uniformity in the flow of water at any one point, as by an error of observation, provided the measurement is conducted with due and proper care. In canals, such variation between two consecutive readings should never exceed five per cent., equal to a variation in the velocity of about four and a half per cent., but as a rule it will be found to be either much less or nothing at all.

To return to a consideration of the instrument itself—of moulinets in general—it is to be regretted that some good maker of instruments has not ere this manufactured them, and supplied each instrument with its proper, as above described, table. Inasmuch as a practising engineer is not usually called upon to do anything more than *use* an instrument as it is furnished him by the maker, it ought still less to be expected of him that he should institute a long and extensive series of experiments with every purchased moulinet, before the same can be of any service to him. Besides, he has frequently no time to do so; he is called upon, say late in the Fall, to gauge the flow of water in a canal, at a time when he wakes up every morning in the expectation of finding a three-inch coating of ice over the scene of his operations. Such experiments are also expensive and troublesome. An instrument maker could choose his own time, and could procure a boat, arranged so as to be dragged through a pond or lake, with instrument properly attached, with various velocities, once for all.* And hence it would seem from all this, that these instruments, whether arranged to register telegraphically, or so arranged as to slide up and down along the rod that holds them,† and each one furnished with a proper table of velocities, will, in the future, be used much more frequently than they have been used hitherto.

Boston, March 23, 1869.

* If all moulinets were made precisely in accordance with a fixed pattern or model, it would suffice to make these experiments, with only two or three such, carefully and accurately; the results thus obtained could then be used for all the others.

† When, as has usually been taught, the rod together with the instrument attached is to be lifted up out of the water at every reading, this operation causes even in measuring in a depth of only ten feet, a great deal of labor, loss of time, and danger of injury to the moulinet itself. The whole apparatus could, however, be easily so arranged, that the heavy rod is always to remain upright on the bottom, while only the moulinet is raised out of and lowered into the water, along this rod, at each reading. For small measurements, in mill flumes, etc., an apparatus of this kind is perhaps even preferable, being more portable, to a Henry telegraphic register.

EXTRACTS FROM AN ENGINEER'S NOTE BOOK.

By W. M. HENDERSON, Hydraulic Engineer.

(Continued from page 247).

On the Strength of Steam Boilers.

THE tensile strength of good iron boiler plate, at 80°, is about 56,000 pounds per square inch. Its tenacity will be increased as the temperature rises, under the conditions of generating steam, up to 550° above the freezing point; its maximum strength then is 65,000 pounds per square inch. From this it decreases in direct proportion; at double that temperature it loses one-half. As the temperature of the water in a steam-boiler rarely exceeds 400°, its application to their construction is especially favorable. For the purposes of calculation its average strength may be assumed to be equal to 60,000 pounds per square inch. The deductions to be made for single riveted plates is 44 per cent, for double riveted plates 30 per cent., or, ratio, plate being 100: single riveted 56, double riveted 70. From this data, the bursting pressure, equivalent to the ultimate strength of the single riveted joint, is reduced to 34,000 pounds per square inch, and the double riveted joint, similarly, to 42,000 pounds. It is commonly believed the plates are strongest in the direction of the fibre, but experiments have proved they are about 2½ per cent. stronger crosswise of the fibre; it may therefore be safely assumed the strength of boiler plates are equal in all directions. The strength of the plates will be increased by the amount of hammering and drawing through the rolls they receive, and a decided increase of strength will be obtained by cold rolling under pressure. On the other hand, cold hammering is highly injurious, causing crystallization, and, consequently, impoverishing of the material. After such treatment the plates should be re-heated, and allowed to cool slowly, to recover their strength. The strength of cylindrical boilers subjected to internal pressure vary inversely, as their diameters; whilst there seems to be no general rule regarding variations in their length, the strength is affected so slightly that it may be almost entirely disregarded in practice. The strength of cylindrical flues subjected to external pressure vary inversely as their diameters, and also as their length. In the case of the pressure acting internally, the material of the boiler shell is extended equally throughout all its parts, and its cylindrical form is main-

tained at all stages of the pressure. This is very different when the pressure acts externally; the material being compressed, loses its cylindrical form by crumpling up in longitudinal lines near the middle of its length. This comparatively small portion of the tube then has to resist the main force of compression, since it will be seen the ends of the tube are rigidly held in position by the inflexible heads of the boiler. The pressure producing this collapsing force is always proportional to the longitudinal section of the flue, whilst the part where the collapse will take place is, to a certain extent, independent of the same. To meet this disparity of strength, the flues should be divided into shorter length, increasing their strength in uniformity with that of the exterior shell of the boiler, by riveting at intervals to the joints, T, or angle iron rings, or by constructing them entirely of corrugated iron. This equalization of the powers of resistance of the different parts of a steam boiler is of the utmost practical importance, as any increased strength of the outer shell is of no absolute value, so long as the internal flues are liable to be destroyed by collapse at a pressure less than half that required to burst the shell which envelopes them, the extra thickness of metal in the shell being so much material thrown away, adding nothing whatever to the strength.

In the construction of steam boilers, it seems to have been tacitly admitted that the flues, if not the strongest parts, were at least quite equal in strength to the outside shell, and this would appear to be the case, judging by the natural course of analogous reasoning, such flues being of greatly reduced diameters, in proportion to the shell, and generally constructed of the same thickness of plate. It was only from the occurrence of frequent explosion by their collapse, that attention was directed to the existing discrepancy, which led to a series of experiments, conducted by Mr. Fairbairn, the result of which established the fact that the flues in the Cornish and double-flue description of boilers, were by far the weakest part of the construction, in many cases being only one-third as strong as the outside shell. Another matter developed in the course of these experiments, and one that even now is generally disregarded, is the manner of constructing the longitudinal joints of flues, by lapping them over each other; such a departure from the true circle, which can easily be maintained by making a *butt* joint, with longitudinal covering plates, will impair to a considerable extent their powers of resistance—the loss of strength sustained from this simple matter

has been ascertained to be as 7 is to 10, or nearly one-third. In every construction where tubes have to sustain an external pressure, the cylindrical is the only form to be relied upon, and any departure from it is attended with danger. Elliptical tubes should never be used, as their powers of resistance are reduced in proportion to the amount of departure from the true curve.

In the designing and constructing of steam boilers, care should be taken to so proportion the various parts that they may be, as nearly as practicable, equally strong in every direction. The flues, as has been shown, will require especial attention, being the weakest parts, as at present constructed. Flat surfaces are known to require staying, and the necessary strength can be readily secured by calculation. Still, after all this, a cylinder boiler will be, estimately, just twice as strong, in the longitudinal direction, as in the curvilinear, from a mathematical principle involved in the construction of all cylindrical vessels, *i. e.*, "the areas of circles are to each other as the squares of their diameters." The material offering resistance to rupture in the longitudinal direction being the area of the rim section of the shell, as against the pressure exerted on the area of the boiler head. In the curvilinear direction, it is the area of the longitudinal section of the plates, passing through the axis of the boiler, as against the pressure exerted on the area of the enclosed section. From this, it will be seen that boilers having increased dimensions should also have increased strength in the ratio of their diameters, *i. e.*, one boiler twice the diameter of another should have double the thickness of plates, having to resist double the pressure in the curvilinear direction, and the heads being increased in the proportion of four to one, quadruple the pressure in the longitudinal direction. That this ratio of progression is correct, will be evident by considering that the circumference is doubled with the diameter, and the thickness of the plate also being doubled, gives four times the area of the previous cross section, the power of resistance increasing in strict uniformity with the demand. I have previously stated that the strength of steam boilers, in their longitudinal direction, is, *estimately*, twice that of the curvilinear: it would be precisely so, were it not for the manner in which many cylindrical boilers are constructed. The shell being composed of short cylinders, formed from one plate, with one row of rivets joining the single lap, here it will be seen one side of the plate remains intact, while where these short cylinders are riveted, one to

the other, the full percentage of rivet holes are punched, or drilled out, giving an increase of strength in the curvilinear direction equal to about the double riveted joint, reducing the excess of strength possessed in the longitudinal direction, from 2 to $1\frac{1}{2}$, and by double riveting the longitudinal seam, this will be further reduced to about $1\frac{1}{3}$.

In regard to the strength of the flat stayed surfaces of steam boilers, it has been found, by actual experiments, that at a temperature of 388° Fahr., or 80 pounds steam pressure, it required a force of 8·1 tons to draw out a $\frac{3}{4}$ iron stay screwed into a $\frac{3}{8}$ copper plate, stripping the thread in the latter; 10·7 tons to draw out a $\frac{3}{4}$ iron stay screwed and riveted into a $\frac{3}{8}$ copper plate; and 12·5 tons to draw out a $\frac{3}{4}$ iron stay screwed and riveted into a $\frac{3}{8}$ iron plate.

Ratio Iron and Iron screwed and riveted	1000
" and Copper " " "	856
" " " "	648
Copper and Copper screwed and riveted	576

Philadelphia, April 5th, 1869.

(To be continued.)

ENGINEERING ARCHITECTURE.

By Alf. P. Boller, C. E.

Custom has so entirely separated the professions of the engineer and architect, that the relations between the two are practically lost sight of. The dividing line it is difficult to lay out rigidly, so insensibly does one merge into the other. It is not desirable that either profession should be an accompaniment of the other in the same individual, but it is important that their relative bearings should mutually be better understood. There are many principles in common, which, felt and realized by the engineer and architect, would elevate their respective professions to a higher degree of usefulness. There is no reason in the world why our engineering works, as a rule, should be as devoid as they are of any effort to attract as works of art; or, on the other hand, that modern architecture should so uniformly violate its high position by an unmeaning decoration of often faulty construction.

It is, indeed, not such a long time back that both professions were practised by the same individual, and occasionally painting

and sculpture were added accomplishments. Since then, the progress of a material development, necessitated by an advancing civilization, opened up such a diversified field of intellectual culture as to compel the gradual separation of the sciences and arts. More especially is this true of the latter part of the last and beginning of the present century, when canal, harbor and railway improvements, changed the whole system of commercial intercourse, and made a new people of most of the nations of the earth.

Men made specialties of the several classes of internal improvements, and now engineering is subdivided into distinct branches, as civil, mechanical, hydraulic engineering, and the like, while architecture has to do exclusively with the erection of buildings in conformity with modern requirements, and calls in general terms for simply good construction, combined with good taste. It would be an interesting, and perhaps a profitable speculation, to inquire in what manner the separation of the professions may have produced what is popularly called the "degeneracy of modern architecture;" but it will be more appropriate in this place to consider the result of this separation upon our engineering profession, and to press the importance of giving more attention to such architectural principles as enter into engineering constructions. In an article like this, we can but deal in generalities, which may serve to call attention to an important and too much neglected topic. The treatment of any abstract subject, like the "Æsthetics of Construction," brings us at once into difficulties at its very threshold. The trouble lies in the fact that we cannot all occupy, without much study, the same standpoint of criticism in a science consisting wholly in sensations, involving mental and moral qualities and conditions, that are different in different men. In other words, it is a heart matter, requiring education and practice to give it outward forms and shapes. Be this as it may, there is no question but that engineering education, in this country at least, almost entirely ignores its architectural bearing, and although there is no real excuse for this neglect, one can readily perceive the cause from which it springs. Whether architecture is looked upon as a ramification of engineering, or engineering a branch of architecture (in which case we must give to architecture its widest meaning), modern practice has delegated the engineer to be a pioneer of society and civilization, bending and utilizing the forces of nature to the service of mankind, while the architect follows in his wake, and endeavors to

cater to the refinements of man, in speaking to his soul through an infinity of beautiful forms and colors, and making it rejoice with the elevating influences of pure art. The result of such division is perhaps natural, in that the one accomplishes his ends without any thought of how his work may be a source of pleasure to his fellows, while the other neglects his proper construction for his "art," and fails in his "art" for the lack of good construction. He forgets that one must supplement the other. The one violates good taste, while the other defies good sense. The engineer has a slight advantage over the architect, for he can have good construction and no art, while the architect cannot get good art without good construction to base it upon. The neglect of this harmony shows in engineering works by a certain harshness and rigidity of form, and an ignorance of the power or relation of color, while architectural works lean toward an effeminate and unskillful construction, covered up by an unmeaning or meretricious ornamentation. There is no reason why our public works, our viaducts, bridges, station buildings, and the like, should not be works of art, and not only specimens of constructive skill. It is the throwing of the heart into his work, as well as the head, that we ask of the engineer, and to appeal to men's sense of the good and beautiful, of truth and honesty, as well as to their feelings of wonder at the constructive power that may be evinced.

Mr. Ruskin, in his "Stones of Venice," bases his architectural criticism and appeals upon the axiom, "that there can be no architecture without building, and no *good* architecture without *good* building;" and he uses the word "building" in the sense of "construction." Now, pass the architectural monstrosities of the present day under review. Apply this truism, and then see why we have such bad architecture. The ornamentation or decoration does not grow out of the necessities of the case, but is a great staring burlesque on the construction it tries to conceal. It is in the essential fitness of things that ornamentation becomes pleasing and appropriate, and it is not in plastering a building or any public work with Grecian columns or pilasters, or Gothicising all the arches, that makes "architecture;" and yet a large proportion of the practitioners in this country would have us think so. Again, ornament ought never to be constructed, that is, artistic forms and graceful shapes should never be put on construction, simply because they are such, for the moment that is done, such forms and shapes

cease to become artistic and graceful. The character of the ornamentation should be expressive of a purpose existing in the work. It has the same relation to building (and by building we mean *all* building, whether a bridge, viaduct, or a house) that poetry has to prose. The latter narrates the facts, in language however rough, the former clothes it with beauty and grace, and expresses passion or sentiment, grandeur or sublimity. Why should not the engineer bring these principles of architecture to bear in his own work? Surely, they eminently belong to him, and especially so since the various ramifications of the professions, in the fullest sense, has compelled him to select specialties. The constructive engineer rarely, nowadays, performs the duties of the surveyor, but devotes his time and energies to the execution of bridges and viaducts, the construction of piers, lighthouses, warehouses, and at times the building of ships. It is to these divisions of the profession that our subject ought to be of much interest, and its proper study would result in public works that would be monuments of beauty or grandeur, as well as specimens of constructive skill. In almost all engineering architecture, simplicity of ornamentation is demanded, and, in truth, there are not a few public works in this country that would take but little to have made them truly architectural. There is no need to waste time or labor in a flippant carving or sculpture, especially so since engineering works susceptible of architectural effect are usually seen at a distance, and the beholder is mostly affected by a pleasing unison of parts and of color. In many works, the simple scientific construction and *proportion* of parts, necessary for the performance of the duty of the structure, is all that is required for a pleasing effect, for proportion is a vital element of what we call beauty. Then, again, engineering works are often characterized by a certain massiveness, engendering a feeling of strength and security, which ought never to be dissipated by light, inappropriate mouldings or carving. To extend the practical bearing of the remarks made in this article, will require the writer to trespass upon the space of the *Journal* in a future number.

PUMPING ENGINES.

By Henry P. M. Birkinbine, C. E.

The *Journal* has done good service by giving so much of its space to the discussion of the relative value of different forms of pumping engines.

The articles already published, as well as the practice of engineers, show that upon this subject there is a great diversity of opinion. An engineer of prominence in this country, in a report upon the water supply of a city, makes the following remarks upon pumping engines: "Much examination has been given to this subject; yet much uncertainty remains. Fortunately this uncertainty, so far as the interests of large cities are actually concerned, is of no great practical importance, notwithstanding so much has been said and written to prove the contrary." The earliest utilization of the power of steam was to raise water, and great numbers of engines are now in use for this purpose, in draining land and mines, supplying cities, etc., and are required for many engineering operations. There should, therefore, now be no ignorance or uncertainty upon the subject; surely, there are some forms better adapted than others.

In the closing paragraph of an article on "Cornish Pumping Engines" in the *Journal*, Vol. 47, p. 16, the writer says: "I will only add that there is yet another engine based upon reciprocating parts, without a fly-wheel, which has the capacity of rivalling the Cornish engine in its best results, without the requirements of magnitude—almost immensity—which the Cornish engine demands." If the writer of this article really knows of such an engine, he certainly owes to the profession and the community to give a full description of it, for in the article alluded to, he credits the Cornish engine with a duty of 130,000,000 foot pounds. (He no doubt means 1,300,000 foot pounds.) There is no record of any other form of engine producing so great a duty under the most favorable circumstances.

From the brief description given, we are left in ignorance as to the engine referred to, and as he has not enlightened us, we may be excused for taking the Yankee privilege of *guessing* that he refers to that class of direct acting engines (of which that known as Worthington's patent is the most notorious, though not the best). This *guess* is made because it answers the description, and because

the maker actually claims a high duty for his engine, and professes to have produced results which are truly wonderful when the peculiar construction of the engines is taken into consideration. Having never heard any of the other makers of this class of engine claim extraordinarily high duty for them, it may be well to examine somewhat into these claims.

The authorities of this city have been induced to contract for two of these engines, upon the statement that they produce a higher average duty than Cornish engines. (See Appendix to Journal of Select Council, 1868, page 237.) In the experiments made with the engines of the former class at Cambridge Water Works (see reports upon pumping engines, 1857 and 1859, page 76), a duty of 712,784 foot pounds is claimed. For a pair of engines of this class, at the Charlestown, Mass. Water Works, a duty of 774,445 foot pounds is claimed.

While this is far less than your correspondent gives to the Cornish Engine, still these results, if true, are most extraordinary, considering the many disadvantageous peculiarities for high duty of this class of engine.

So much for the duty claimed. In the Cambridge City Documents, page 137, there is a statement of the expense of running these engines for one month (made as a comparative test to show their economy), when 52,800 gallons of water were raised one foot high by one pound of coal; this gives a duty of but 440,000 foot pounds. This is more than could be expected from such a form of engine, although it is much less than the duty claimed for it. The peculiarities by which it is *claimed* that the high duties of this form of engine are produced, are, the duplicating of the pump and engine, and arranging the duplex machinery so that one engine works the valves of the others, thereby producing a comparatively continuous flow from the pumps, and high degree of expansion, particularly in those having double steam cylinders.

The only possible advantage of duplicating the engines is a more steady flow of water in the delivery pipe; but as duplicating the parts increases the friction, and there is unavoidable loss of steam in ports, cylinder ends, etc., it certainly can effect no actual gain in duty.

The stroke of engines of this class is of necessity very short; a double acting pump of the diameter that experienced engineers would make stroke of five (5) feet, is rarely over two (2) feet, thus

necessitating the more frequent stopping and starting of the water at the commencement of each stroke, another element impairing their efficiency.

Much stress is laid upon the economy produced by the expansion, particularly when the double cylinder or Wolf engine is used; but by this there can be but little gained, as there is neither fly-wheel, beam, or other heavy moving parts in which to store the excess of power in the earlier stages of the stroke, and utilize it towards the close of the same; the only reservoir of force being the water in motion and the difference between the power necessary to start the column of water, and to keep it moving. The terminal pressure upon the piston in these engines must be sufficient to keep the column of water in motion, whereby a large amount of force is lost which may be utilized in engines with fly-wheels, or other heavy moving parts.

In practice, very few of this class of engines make their full journey; frequently there is a large amount of steam which cannot be used, left in each end of the cylinder. From the above, it is evident that such an engine which gives an actual duty of four hundred thousand (400,000) foot pounds, is truly a remarkable machine.

So far as the writer has been able to examine them, none have produced an actual duty of two hundred thousand (200,000) foot pounds which (considering the peculiarities of their construction), is a fair average duty. It is doubtful if any of them, no matter by whom constructed, can exceed that duty.

The engines of this class are found to be valuable where a small amount of water is to be raised, or where they are only used occasionally.

One form is extensively employed in the Pennsylvania anthracite coal mines, and is found to be a desirable arrangement; the pump being placed at the bottom of the mine, long connecting rods are not required, thus saving the friction incident upon carrying power so great a distance. The coal used having but a nominal value, economy of fuel is not an object. But where an amount of over one million of gallons of water per day is to be raised one hundred feet, the question of fuel becomes the one of the greatest importance.

From a careful examination of the recorded duty, and a personal inspection of many of the various forms of pumping engines, the

following facts upon the subject of duty in foot pounds have been collected.

	Highest Duty claimed.	Average Actual duty.
Cornish Pumping Engines	1,300,000	550,000
Condensing Pumping Engines with fly-wheel	700,000	350,000
High Pressure Pumping Engines with fly-wheel	250,000	200,000
Direct Acting Engines of the Worthington type	750,000	200,000

By making an estimate (from the above actual duties) of the quantities of coal required to raise, say, five million gallons one hundred and fifty feet high, the relative value of the different forms of pumping engines will be apparent. It is as follows:—

Cornish Engine	11,427	pounds of coal.
Condensing fly-wheel engine	17,959	" "
High pressure fly-wheel engine	31,425	" "
Direct acting engine, as above	31,425	" "

The other items of running expenses are about in the same ratio; the first cost being the only element in which a large difference could exist. The value of engines is much controlled by the style of finish and peculiarities of construction. Thus, the cost of an overhead beam Cornish pumping engine is very much greater than that of a Bull or upright Cornish engine of the same capacity and efficiency.

The saving of fuel alone will indicate that at a liberal estimate for the cost of the first two styles of engines mentioned, the last two would be dear at any price.

Patents and royalties have no doubt much to do with the apparent diversity of opinion as to the best form of pumping engine; some of them would most certainly never be considered, if not pressed into notice by those having pecuniary interests in their success. The authorities of one of our important cities, requiring a large supply of water, were actually induced to take into serious consideration the adoption of a patented form of rotary steam engine and rotary pump, and had not a prominent citizen conversant with such matters, exerted himself and enlightened the public, the construction of a four hundred and fifty (450) horse power rotary

pumping engine would have been attempted, for which the patentees promised excessive duties, and many other advantages over all other forms of pumping engines. The pertinacity with which many patents are urged upon the public, the combinations of men and money formed to press them, and the difficulty of arriving at the actual facts in regard to the duty and efficiency of the various forms of pumping engines, make it important that the subject be thoroughly understood, so that the best and most efficient form of engine may be adopted.

BELTING FACTS AND FIGURES.

By J. H. Cooper.

(Continued from page 238.)

"Among the more recent improvements in the way of *transmitting power* for long distances, is the substitution of belts by endless wire-ropes, running at a high speed; their use bids fair to add immensely to our manufacturing facilities. The distance to which you can thus transfer power ranges from 75 feet to 4 miles. Just where the belt becomes too long for economy, there the rope steps in. In place of a flat faced pulley, a narrow sheave with a deep flaring groove is used, the groove being filled out, or lined rather, with leather, oakum, india rubber, or some other soft substance, to save the rope. The essential points are a large sheave, running at a considerable velocity, and a light rope.

When the distance exceeds 400 feet, a double grooved wheel is used, and a second endless rope transmits the power 400 feet further, and so on indefinitely. The loss by friction is about 8 per cent. per mile. A few examples may prove of interest, and give information.

It is required to transmit 300 horse-power by means of a wire rope. A wheel 14½ feet diameter, making 108 revolutions per minute, is sufficient; the rope running at a rate of 4920 feet per minute—size of rope required, one inch diameter. The distance has nothing to do with it. Again: "It is desired to transmit for any distance as much power as a 12 inch belt will give." Assuming that the belt travels in the neighborhood of 1300 feet per minute, it is about equivalent to 20 horse power, and a grooved sheave of 7 feet diameter running 100 revolutions per minute, with a ⅝-inch

rope, will be the proportions required. Again, a 4 feet wheel, running 100 revolutions per minute, with a $\frac{3}{8}$-inch rope, will convey from 4 to 5 horse-power. The cost of the rope is always the smallest item, amounting to a few cents per foot, and not one-tenth the cost of an equivalent amount of belting.

"One is thus enabled, at a small expense, to transmit power in any direction; for instance, to a building lying remote from the main factory buildings, where it is not worth while to put up a separate engine. Across rivers, creeks, canals, streets, over the tops of houses, under water, from cellar to roof, etc.

Frequently an excellent site for water power remains unimproved for want of suitable building sites in the neighborhood. The water may be conveyed *down stream* by means of expensive canals and flumes; but by a wire rope transmission we can transfer it in any direction, either up stream, across it, or sidewise, up and down grades of one in eight—in fact, anywhere.

"In many sections of our country coal is dear and water power plenty, but not improved, for reasons which may be set aside by the above method. In Europe, over a thousand factories are driven in that way."—*The Manufacturer and Builder*, Feb. '69, p. 38.

In a paper by Mr. John Ramsbottom, in *Newton's Journal*, Vol. XXI, p. 46, on traversing Cranes at Crewe Locomotive Works, dated January 28, 1864, mention is made of the means by which power is communicated from the shop lines of shafting to the gear of the cranes.

"It consists of a $\frac{5}{8}$-inch diameter soft, white cotton cord, weighing about $1\frac{1}{2}$ ounces to the foot, running at the rate of 5000 feet per minute, in a line with the longitudinal motion of the crane, above the same and over a 4 feet diameter tightener sheave. This sheave is weighed so as to put a tension on each strand of the cord of 108 pounds, which is found to be the best working strain for keeping the rope steady, and giving the required 'hold' on the main driving pulley.

The cranes have a span of 40 feet 7 inches, a longitudinal traverse of 270 feet, and the rails are 16 feet above the floor.

The cord is supported every 12 or 14 feet by cast iron fixed slippers of plain tough section, $1\frac{3}{8}$-inch wide, with side flanges. These slippers are placed $1\frac{1}{2}$ inch below the working line of the driving side of the cord, so as to allow the driving wheels on the traverser

to pass them ; they are not oiled, and the friction of the cord in them amounts to two-fifths of the working load.

Motion is communicated to the gear of the crane by pressing the cord into grooved cast iron pulleys. The grooves in the driving pulleys are V shaped, at an angle of 30°, and the cord does not touch bottom ; the guide pulleys have circular grooves, same diameter as the cord, and the pressure pulleys have a circular groove of larger diameter than the cord. The driving pulleys have a diameter equal to thirty times the diameter of the rope. Guards are put on the pulleys to keep the ropes in.

The driving power of the cord to lift 25 tons is only 18 pounds, irrespective of friction, which is a ratio of 3111 : 1. Light loads are about 800 : 1. In the gib cranes, driven by similar means, the ratio is 1000 : 1 when lifting 4 tons at the rate of 5 feet $1\frac{1}{2}$ inch per minute.

The actual power required to lift 9 tons, besides the snatch block and chain, has been found to be 17 pounds at the circumference of the driving pulley. The crab, when unloaded, requires $1\frac{1}{8}$ pounds to overcome its friction.

The cords are soon reduced to $\frac{9}{16}$ inch diameter, and last about eight months at constant work.

In an overhead traverser, used in the boiler shop, lifting 6 tons, three years in use, a $\frac{3}{8}$-inch cord was employed, but was afterwards changed for a cord $\frac{1}{2}$-inch in diameter.

The light driving cord is the only plan compatible with high speeds ; a heavy chain, belt or cord would soon wear out and break by its own weight.

Centrifugal Pumps.—The great Appold centrifugal pumps, to be worked in connection with Mr. Hawkshaw's important work, the Amsterdam Ship Canal, are to lift 2,000 cubic metres, or, say, 440,000 gallons of water per minute. The lift is not great, but for each foot of lift, the actual duty, irrespective of all losses of effect, is $133\frac{1}{3}$ horse-power.—*Engineering.*

Mechanics, Physics, and Chemistry.

ON THE SOURCE OF LIGHT IN LUMINOUS FLAMES.

By Edward Frankland, F.R.S.

(Professor of Chemistry, R. I.)

The most prolific source of error amongst mankind is the unquestioning acceptation of authoritative opinion. However much we may pride ourselves upon the sifting of the explanations of things by our own enlightened judgments, it cannot be denied that the *ipse dixit* mode of settlement is still wonderfully frequent amongst us. Not only is this the case with the public in general, but even the cultivators of science are not entirely innocent of the same weakness.

The essential difference between a fact and a theory is not always appreciated with sufficient vividness. The statement that "16 parts by weight of oxygen unite with 2 parts of hydrogen to form water," is considered by many, for instance, as perfectly synonymous with the assertion that "1 atom of oxygen unites with 2 atoms of hydrogen to form water."

The existence of an imponderable ethereal medium filling all space is often regarded as equally certain with the presence of a gaseous envelope surrounding our globe.

The atomic theory and the hypothesis of an ethereal medium are, at present, absolutely necessary, the one to the progress of chemistry, the other to the further development of physics; but neither this circumstance nor the splendid discoveries made by their aid can establish their truth. A mathematician starting from false data is sure to arrive at a false result; but it is far otherwise with theory, for false theories can, and constantly do, conduct to true facts. Thus Columbus's counterpoise theory of the earth led to the discovery of America, although that theory was, nevertheless, essentially false.

The most sober worker in science cannot progress without the assistance of theory to co-ordinate his facts, and to lead him on to further research. It is here that even a false theory is invaluable, and it is only when the theory continues to be held after it has become opposed to facts, that it exercises a prejudicial influence

upon the progress of science. Then it hinders rather than expedites the advance of the experimenter, and ought to be at once abandoned.

In pursuing the investigation forming the subject of this discourse, the speaker had been compelled thus to abandon a theory of the source of light in luminous flames, which he, in common with others, had derived from Davy's classical researches on flame.

Our text-books answer the question, *What is the source of light in a luminous gas or candle flame?* in the most positive and unanimous manner.

Selecting from some of the most celebrated, the following quotations may be made:—

"All our artificial lights depend upon the ignition of solid matter, in the intense heat developed by the chemical changes attendant on combustion."—*W. A. Miller.*

"Whenever hydrocarbons are imperfectly burnt, there is a deposition of carbon, and this temporary deposition of carbon is an *essential* condition for the production of the white light required in an ordinary flame."—*Williamson.*

"The illuminating power of the gas flame is, therefore, due to these *carbon particles*, which are afterwards burned nearer the border of the flame."—*Balfour Stewart.*

"The brightness or illuminating power of flame depends not only on the degree of heat, but likewise on the presence or absence of solid particles which may act as radiant points. A flame containing no such particles emits but a feeble light, even if its temperature is the highest possible."—*Watts.*

The speaker then proceeded to investigate a number of different flames: he showed that there are many flames possessing a high degree of luminosity, which cannot possibly contain solid particles. Thus the flame of metallic arsenic, burning in oxygen, emits a remarkably intense white light; and as metallic arsenic volatilizes at 180° C., and its product of combustion, arsenious anhydride, at 218° C., whilst the temperature of incandescence in solids is at least 500° C., it is obviously impossible here to assume the presence of ignited solid particles in the flame. Again, if carbonic disulphide vapor be made to burn in oxygen, or oxygen in carbonic disulphide vapor, an almost insupportably brilliant light is the result; now, fuliginous matter is never present in any part of this flame, and the boiling point of sulphur (440° C.) is below the

temperature of incandescence, so that the assumption of solid particles in the flame is here also inadmissible. If the last experiment be varied by the substitution of nitric oxide gas for oxygen, the result is still the same; and the dazzling light produced by the combustion of these compounds is also so rich in the refrangible rays, that it has been employed in taking instantaneous photographs, and for exhibiting the phenomena of fluorescence. Lastly, amongst the chemical reactions celebrated for the production of dazzling light, there are few which surpass the active combustion of phosphorus in oxygen. Now, phosphoric anhydride, the product of this combustion, is volatile at a red heat,* and it is, therefore, manifestly impossible that this substance should exist in the solid form at the temperature of the phosphorus flame, which far transcends the melting point of platinum.

For these reasons, and for others which the speaker had stated in a course of lectures on "Coal Gas," delivered in March, 1867, and printed in the *Journal of Gas Lighting*, he considered that incandescent particles of carbon are not the source of light in gas and candle flames, but that the luminosity of these flames is due to radiations from dense but transparent hydrocarbon vapors, As a further generalization from the above-mentioned experiments, he was led to the conclusion that dense gases and vapors become luminous at much lower temperatures than aeriform fluids of comparatively low specific gravity; and that this result is, to a great extent, if not altogether, independent of the nature of the gas or vapor, inasmuch as he found that gases of low density, which are not luminous at a given temperature when burnt under common atmospheric pressure, become so when they are simultaneously compressed. Thus, mixtures of hydrogen and carbonic oxide with oxygen emit but little light when they are burnt or exploded in free air, but exhibit intense luminosity when exploded in closed glass vessels, so as to prevent their expansion at the moment of combustion.

In a communication just made to the Royal Society, the speaker

* Davy mentions this fact in connection with his view of the source of luminosity in flames, and endeavors to explain the (to him) anomalous phenomenon. He says: "Since this paper has been written, I have found that phosphoric acid volatilizes slowly at a strong red heat, but under a moderate pressure it bears a white heat; and in a flame so intense as that of phosphorus, the elastic force must produce the effect of compression."—*Davy's Works*, vol. vi., p. 48.

had described the extension of these experiments to the combustion of jets of hydrogen and carbonic oxide in oxygen under a pressure gradually increasing to twenty atmospheres. These experiments, which were conducted in the laboratory of the Royal Institution, were made in a strong wrought-iron vessel furnished with a thick glass plate of sufficient size to permit of the optical examination of the flame. The appearance of a jet of hydrogen burning in oxygen under the ordinary atmospheric pressure was exhibited. On increasing the pressure to two atmospheres, the previously feeble luminosity was shown to be very markedly augmented, whilst at ten atmospheres' pressure, the light emitted by a jet about one inch long was amply sufficient to enable the observer to read a newspaper at a distance of two feet from the flame, and this without any reflecting surface behind the flame. Examined by the spectroscope, *the spectrum of this flame is bright and perfectly continuous from red to violet.*

With an higher initial luminosity, the flame of carbonic oxide in oxygen becomes much more luminous at a pressure of ten atmospheres, than a flame of hydrogen of the same size and burning under the same pressure. The spectrum of carbonic oxide burning in oxygen under a pressure of fourteen atmospheres is very brilliant and perfectly continuous.

If it be true that dense gases emit more light than rare ones when ignited, the passage of the electric spark through different gases ought to produce an amount of light varying with the density of the gas; and the speaker showed that electric sparks passed as nearly as possible, under similar conditions, through hydrogen, oxygen, chlorine and sulphurous anhydride, emit light, the intensity of which is very slight in the case of hydrogen, considerable in that of oxygen, and very great in the case of chlorine and sulphurous anhydride. On passing a stream of induction sparks through the gas standing over liquefied sulphurous anhydride in a strong tube, at the ordinary temperature, when a pressure of about three atmospheres was exerted by the gas, a very brilliant light was obtained. A stream of induction sparks was passed through air confined in a glass tube connected with a condensing syringe, and the pressure of the air being then augmented to two or three atmospheres, a very marked increase in the luminosity of the sparks was observed, whilst on allowing the condensed air to escape, the same phenomena were observed in the reverse order.

Way's mercurial light was also exhibited as an instance of intense light by the ignition of the heavy vapor of mercury.

The gases and vapors just mentioned have the following relative densities:—

Hydrogen	1
Air	14·5
Oxygen	16
Sulphurous anhydride	32
Chlorine	35·5
Mercury	100
Phosphoric anhydride	71 or 142

The feeble light emitted by phosphorus when burning in chlorine seems, at first sight, to be an exception to the law just indicated, for the density of the product of combustion (phosphorous trichloride) 68·7 would lead us to anticipate the evolution of considerable light. But it must be borne in mind that the luminosity of a flame depends also upon its temperature, and it can be shown that the temperature in this case is probably greatly inferior to that produced by the combustion of phosphorous in oxygen. We have not all the necessary *data* for calculating the temperature of these flames, but, according to Andrews, phosphorus burnt in oxygen gives 5747 heat units, which, divided by the weight of the product from one grain of phosphorus, gives 2500 units. When phosphorus burns in chlorine, it gives only (according to the same authority) 2085 heat units, which, divided as before by the weight of the product, gives 470 units. It is, therefore, evident that the temperature in the latter case must be greatly below that produced in the former, unless the specific heat of phosphoric anhydride be enormously higher than that of phosphorous trichloride. The speaker had, in fact, found that if the temperature of the flame of phosphorus, burning in chlorine, be raised about 500° C. by previously heating both elements to that extent, the flame emitted a brilliant white light.

To return to ordinary luminous flames, the argument of the *necessity* of solid particles to explain their luminosity obviously falls to the ground; and a closer examination into the evidence of the existence of these particles reveals its extreme weakness. Soot from a gas-flame is not elementary carbon, it always contains hydrogen. The perfect transparency of the luminous portion of flame also tends to negative the idea of the presence in it of solid particles. The continuous spectrum of gas and candle-flames does not require,

as is commonly supposed, the assumption of solid particles. The spectra of the flames of carbonic oxide in air, of carbonic disulphide, arsenic, and phosphorus in oxygen, are continuous, and so, as we have seen, is that of hydrogen burning in that of oxygen under a pressure of ten atmospheres. It is to the behavior of hydrocarbons under the influence of heat that we must look for the source of luminosity in a gas-flame. These gradually lose hydrogen, whilst their carbon atoms coalesce to form compounds of greater complexity, and, consequently, of greater vapor density. Thus marsh-gas ($C H_4$) becomes acetylene ($C_2 H_2$), and the density increases from 8 to 13. Again, olefiant gas ($C_2 H_4$) forms napthaline ($C_{10} H_8$), when the vapor density augments from 14 to 64. These are some of the dense hydrocarbons which are known to exist in a gas-flame; but there are, doubtless, others still more dense; pitch, for instance, must consist of the condensed vapors of such heavy hydrocarbons. for it distills over from the retorts in the process of gas-making, Candle-flames are similarly constituted. The direct dependence of the luminosity of gas and candle-flames upon atmospheric pressure, also strongly confirms the view that the light of these flames is due to incandescent dense vapors.

This inquiry cannot be confined to terrestrial objects. Science seeks alike for law in the meanest and grandest objects of creation. From questioning a candle she addresses herself to suns, stars, nebulæ, and comets; the same considerations which have just been applied to gas and candle-flames are equally pertinent to these great cosmical sources of light.—*Proceedings of the Royal Institution.*

A NEW ARRANGEMENT OF THE HOLTZ MACHINE.

By Prof. H. L. Smith.

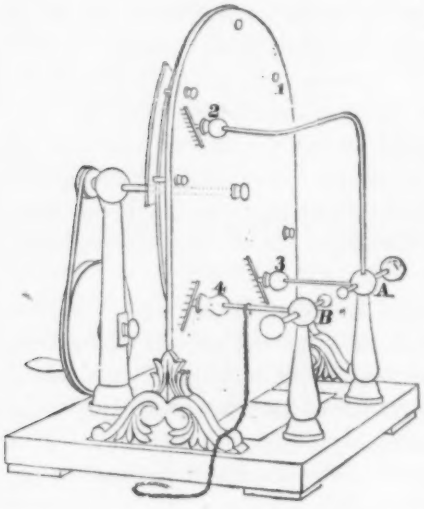
In the ordinary mode of excitation of the Holtz Machine, the knobs of the dischargers are placed in contact, and afterwards separated; the excitation cannot be produced without contact. It follows that, when the balls are separated beyond the striking distance, all action will suddenly cease, and to renew it the balls must again be brought together; and, frequently, in addition to this, a new excitation of the sectors will be required.

A large class of electrical experiments require, virtually, this

separation of the balls beyond the striking distance, *e. g.*, the "electrical flyers," the "head of hair," and the "insulating stool."

The arrangement which I have devised meets this difficulty, and enables us to keep up an unlimited excitation, whatever be the number of points applied to the prime conductor, or whatever may be the distance of the balls of the dischargers; and, in order to excite the machine, it is not necessary that the balls should be in contact.

The arrangement is shown in the following cut. The combs, 2 and 3, are connected with the discharger, A, the one by means of a curved brass bar, the other by a short, straight rod. The prime conductor, which should be large to obtain brilliant effects, and which is not shown in the cut, is placed in contact with the curved bar. Mr. Ritchie places a ball on this bar, with an opening for the insertion of a rod connected to the prime conductor. Opposite to the combs, 2 and 3, are two sectors; that opposed to 2 is shown in the cut. The comb, 4, is connected with the pillar, B, and there is no sector opposed to it. A chain, shown in the cut, connects B with the earth, and it is essential that this connection be as complete as possible, such, for example, as would be made by connecting with the gas pipe. To start the machine, the excited vulcanite may be applied to either of the sectors, 2 or 3; if to the latter, which has the paper tongue of the "feeder" towards the "earth comb," 4, the prime conductor will be charged with positive electricity, and with a 20 or 24 inch plate, sparks $3\frac{1}{2}$ to $4\frac{1}{2}$ inches in length will be seen to pass between the balls of the discharging rods. If the other sector, 2, which has the tongue turned away from the earth comb, is excited, the conductor will be charged with negative electricity, and the sparks will be much shorter and denser. In either case, the length and brilliancy of the spark will depend on the size



of the conductor. When in action, one of the sectors will be +, the other —, and the connection of a + and — sector with the same prime conductor makes the analogy to an electrophorus more clear, the two connected with the conductor representing the upper and under surface of the cover in opposite conditions.

I do not intend, however, to discuss, at present, the theory of this remarkable machine; there is much about it which, with our present knowledge, cannot be satisfactorily explained. The paper tongues may be replaced by needles; and in the arrangement I have described, the sector opposed to comb 3 may be removed (after first exciting the plate by means of sector 2) and the hand held in its place, sparks, not so long, indeed, or frequent, but still sufficiently remarkable, will pass between the balls of the dischargers.

Hobart College, Geneva, N. Y.

THE ELECTRIC SPARK—SOME NEW EXPERIMENTS BY M. J. SEGUIN.

Translated from *Les Mondes*, Vol. XIX., p. 112.

MANY physicists have endeavored to distinguish between the two parts of the induction discharge which are called the halo and spark. The difference between them can easily be increased or diminished by changing the condition of the experiment, as by varying the nature and distance of the electrodes, and the nature and pressure of the surrounding gas. Amongst the most distinct characteristics, there is, besides the difference in appearance, the difference in the action of the two parts of the discharge under the influence of a current of air, or of an electro-magnet, and the difference as seen in a revolving mirror. But all these characteristics are effaced by the variations of the gaseous medium, and it is in this connection that I would here add some experiments to those which I have already described.

1st. Let a spark of one or many centimetres be produced by means of a Ruhmkorff coil between the points of two platinum wires in the interior of a bell jar, which may be exhausted by an air pump. At the first stroke of the piston the halo becomes brighter, and the central spark diminishes in brightness, and at last disappears. The spark is only a luminous jet, of red color, which seems to come from the positive electrode. It is useless to dwell on these changes,

which are well known. But this luminous jet has all the characteristics which belong exclusively to the halo before the rarefaction. I repeat that it deviates by the action of the magnets, and yields equally to the action of a transverse current of air. The current of air is obtained by means of two tubes entering the bell-jar, a large one for exhaustion and a small one through which the exterior air enters. The luminous jet is inflected its entire length as the tube for aspiration is introduced, and it is even divided into two parts by the current of air.

2d. I have observed, with a rotating mirror, the image of the luminous jet when the spark has become effaced. M. Fernet has described the widening which occurs in this case to the blue light of the negative electrode; the red jet which springs from the positive electrode is spread out in the same way, and it is thus seen that the incandescence lasts for some time in all discharges, as M. Lissajous has described in the case of the halo.

3d. The influence of the magnet was tried on a spark produced across the heated column of air which rises above an ordinary flame. The spark has nearly the same appearance as in rarefied air; and if it is made to spring transversely between the poles of an electro-magnet, it is seen to bend on a plane perpendicular to the line of the poles as long as the magnet is in action. The direction of the curve depends upon the direction of the magnetic poles. The spark in this experiment was weak, and in the heated air was only a centimetre long.

4th. As the two parts of the spark pass into each other so easily, it is not astonishing that physicists have not been able to pronounce in a positive manner, when they endeavored to discover whether to attribute to the surrounding medium, or to the substance of the electrodes, the noise of the spark and the light of the halo. Generally they have proved in each of these parts the presence of matter. The preponderating influence depends upon the same circumstances which vary the aspect of the discharge, and yet this has not the same character in all parts of its length. For my part, I have particularly observed the influence of the surrounding gas on the spark, as I have often observed that the noise, the color, and the prismatic characteristic of the spark changes with the nature of the gas. On the other hand, I admitted the intervention of the matter of the electrodes in the halo, after having observed that the spark produced between a platinum wire and an amalgamated cop-

per wire gives in the halo the bright lines due to mercury, and that the halo was also colored with various tints in the sparks which pass between a platinum wire and the surface of saline solutions, to such a degree that a breath expanded the halo into a broad colored sheet, and giving the bright lines due to the base of the salt when viewed with the spectroscope.

These last experiments, repeated under different conditions, very plainly show the influence of the surrounding medium upon the character of the discharge. The platinum wire and the solution are placed as M. Ed. Becquerel suggests, and the solution is connected with the negative pole, as this pole is the most favorable for coloring the spark, which is contrary, however, to a statement I have already given. Then the wire and the liquid are placed in a bell-jar, where the air can be rarefied. By this rarefaction the brilliant spark loses itself in the halo which is colored by the salt, and the color becomes fainter on the upper part of the spark, and draws nearer the solution. The spectral lines of the salt finally disappear, or are only seen intermittently. Other lines than those due to the salt remain in the image of the spark.

M. Ed. Becquerel having just called the attention of physicists to the part which spectrum analysis can undertake in the study of the sparks produced at the surface of solutions of salt, allow me to recall, that in making the discharge, not on the surface of solutions, but as M. Daniel made it, across non-conducting liquids, and adding to these liquids salts in powder, or in solution, I have observed the lines due to the base of each salt.

Inverted Syphon.—An iron pipe, 11 inches in diameter and 8,800 feet (one and two-third miles) long, has been laid in Tuolumne county, California. It runs down a mountain, under a creek, and up the ascent on the opposite side, under a perpendicular pressure at the lowest point of 684 feet.

Water Supply for Jerusalem.—Miss Burdett Coutts has taken on herself the entire expense of introducing pure water into Jerusalem.

THE SALT DEPOSITS AT STASSFURT.

By Messrs. BALD AND MACTEAR.*

THE southern portion of the North German Basin is divided by the Hartz into two portions, which are known as the Thuringian and the Magdeburg Halberstader basins, in which salt has been raised for a lengthened period in the form of brine.

The basin covers a surface of 120 English square miles, and is filled with new red sandstone, which is not broken up by any of the older formations. It is interspersed by elevations of gypsum, which is considered a certain indication of the presence of common salt. In the Prussian mine at Stassfurt, in the Magdeburg basin, after passing through 27 feet of alluvial soil, a thickness of 576 of new red sandstone is at once reached, then 213 feet of gypsum, anhydrite and marl, the salt being found at a depth of 816 feet. In the Anhalt mine (half a mile from the Prussian one) the sandstone is entirely wanting, the salt bed being reached at a depth of 480 feet, after passing through 20 feet of soil and 460 feet of gypsum, anhydrite, and marl. The bore-holes at Schonebeck (some miles from Strassfurt) show very distinctly the various strata with which the basin is filled up, as the salts gradually get deeper and deeper. Thus, at bore No. 8, the salt is 1000 feet from the surface, the intervening strata being 200 feet of alluvial soil and 800 feet of new red sandstone. At No. 5 there is 37 feet of alluvial soil, 166 feet of mussel-chalk, and 1277 feet of new red sandstone, the salt being 1480 feet from the surface. At No. 6 there is 30 feet alluvial soil, 877 feet mussel-chalk, and 473 feet new red sandstone, the salt being 1380 feet from the surface. At No. 4 there is 25 feet alluvial soil, 211 feet of what in Germany is called keuper and lettenkohle (literally, copper and letten coal). This keuper is the equivalent of the saliferous and gypseous shales and sandstones of Cheshire, a member of the "Irias" or new red sandstone formation. Lettenkohle is a variety of lignite known in the district as brown coal. Next we have 1067 feet of mussel-chalk, 377 feet of new red sandstone, and the salt at a depth of 1680 feet. Bore No. 3 is somewhat similar to No. 4, there being 30 feet alluvial soil, 435 feet keuper and let-

* Read before the Chemical Section of the Glasgow Philosophical Society, January 18, 1868.

tenkohle, 1087 mussel-chalk, 212 new red sandstone, the salt being 1764 feet from the surface.

In the Magdeburg basin the salt rests on new red sandstone, and in the Thuringian basin on mussel-chalk and magnesian limestone.

It is only at Stassfurt and Erfurt that the salt is mined; at all the other places it is obtained by means of brine wells, the liquor from which is concentrated by the graduation process, which consists in allowing the weak liquor to trickle through walls made of bundles of thorns and brushwood.

The graduation houses consist of a timber framing, into which the faggots of thorns are built in regular walls. The structure is covered with a roof to protect it from the rain, but the sides, of course, are open to admit of the free passage of air, which, together with the solar heat, forms the evaporating medium.

The walls are from 30 to 50 feet high, and of immense length, the celebrated one at Schonebeck being fully more than an English mile in length. They are placed in the manner best suited to obtain the full benefit of the prevailing wind. The house is divided into several sections, and the weak liquor is pumped into a cistern, from which it is led by means of a perforated pipe along the top of the first division, down the sides of which it trickles into a large wooden tank underneath. From this it is pumped up and allowed to trickle through the second division, from underneath which it is pumped on to the third, and so on until it reaches the last one. In graduation houses, where the number of compartments does not exceed three, and, indeed, in all of them, to a greater or less extent, the liquor is pumped through the same division several times. The weak brine at Schonebeck contains $7\frac{1}{2}$ per cent. of common salt, which, at the finish of the graduation process, is raised to about 22 per cent. In this state it is run into large tanks, of which there are eight at Schonebeck, of an aggregate capacity of about two and a half million gallons. From these tanks it is drawn off to the evaporating pans as required for boiling down. At these works the process of graduation can be carried on for an average of 250 days in the year.

The boring operations were commenced at Stassfurt on the 3d of April, 1839, and in June, 1843, had penetrated to the rock salt region. In January, 1851, when it had reached a depth of 1851 feet, the liquor from the bores contained—

Sulphate of magnesium	4·01
Chloride of magnesium	19·43
Chloride of potassium	2·24
Chloride of sodium	5·61
Total salts	31·29

However, in 1848, Professor Marchand gave it as his opinion that the salts were not mixed in the manner represented by the brine, but that pure rock salt would be found at the bottom with the more soluble salts overlying it; and so much weight was given to his opinion that in December, 1851, after having penetrated to a depth of just as many feet as there were then years in the Christian era, the sinking of the shaft "Von der Heydt" was commenced, followed in January, 1852, by that of the shaft "Von Monteuffel;" and in 1856 the pure salt was found 1066 feet from the surface.

The shaft passes through—1st, 27 feet of alluvial soil; 2d, 576 feet of sandstone, with some schist and grey limestone; 3d, 192 feet of gypsum and anhydrite; 4th, 21 feet of bituminous matter mixed with anhydrite and common salt—making in all 816 feet. Next there is 158 feet of abram or potash salts, the value of which was not recognised at first, but which now play a very important part in the industry of the country. The shaft then passes through 92 feet of rock salt, the upper portion of which is rather impure, being mixed to a considerable extent with anhydrite. This makes a total depth of 1066 feet, and at this point the lateral workings were commenced. These consist of large galleries, the principal of which are from 40 to 60 feet broad, 20 to 25 feet in height, and about 200 feet long.

The salt is wrought in a manner somewhat similar to our long wall system, a series of holes of sufficient depth, about 6 feet or so, are drilled in the face of the salt about 5 feet from the floor, and this depth of material is removed by a series of small blasts. This operation is repeated until a considerable space has been cleared under the overhanging mass of salt. Bore-holes are then drilled close to the roof, and by a series of simultaneous blasts, a large mass of salt is dislodged. In one of those halls or galleries which we visited there was lying on the floor a mass of between two and three thousand tons which had been removed in this manner a few days previously. A number of boys are employed to pick out the pieces of pure salt, which only requires grinding to fit it for do-

mestic use. The salt is removed to the pit bottom in hutches running upon rails, exactly similar to those in use in our own coal pits. From this they are lifted to the surface by an engine of 130 horse-power, and removed to the grinding-mills, of which there are twelve at the mines. There is also a 200 horse-power engine for pumping, which lifts 13 cubic feet of water per minute.

The workings into the potash salts are opened on the other side of the shaft from the common salt galleries, for although the salts are deposited one on the top of the other, still as they dip at an angle of 30°, they are all wrought from the one level.

The total thickness of the salts is 1197 feet, and this may be said to consist of—

Rock salt	989 feet.
Anhydrite	36 "
Polyhalite	13 "
Kieserite	51 "
Carnallite	98 "
Hydrated chloride of Magnesium	13 "

This gives a composition of—

Chloride of sodium	85·82
Sulphate of calcium	4·88
Sulphate of magnesium	4·70
Sulphate of potassium	0·40
Chloride of magnesium	2·53
Chloride of potassium	1·67
	100·00

—*Chemical News.*

(To be Continued.)

Lightning in a Mine.—We see in *Les Mondes* an account of a lightning flash which struck the hoisting machinery over a mine, descended the shaft for some 1800 feet, and then passed along a lateral gallery for 3600 feet more, doing various injury, by the way, before it was lost in the body of the earth. Such an action is, we believe, altogether unprecedented, and indicates a curious combination of condition by which the electric discharge could find a better conductor in the opening of the shaft and gallery than in the surrounding earth.

EDUCATIONAL

SUNLIGHT AND MOONLIGHT.

By Prof. Henry Morton, Ph. D.

(Continued from page 275.)

Passing outward from the solar photosphere or visible surface which we have described in its main features, we come to the relatively non-luminous atmosphere, by which this is surrounded to an unknown distance. In presence of the great amount of diffused light reflected from our atmosphere, we can, of course, learn little or nothing as to objects in the immediate vicinity of the sun, and it is, therefore, mainly to observations made during the rare occurrences of a total eclipse, that we owe such knowledge as we possess on this subject.*

During the continuance of "totality," that is, the time when the luminous solar disk is entirely covered by the moon, a halo is seen stretching off for an immense distance into space. This is irregularly illuminated, giving the appearance of rays, and has occasionally been observed to possess a curious shape as that of a Greek cross in the eclipse of 1860, and an hour glass in 1842. Observations with the polariscope show that the light of this halo, which is called the "corona," is polarized in planes passing through the sun's centre, which would be the case if it were sunlight reflected from particles of matter at various distances about the sun.

It is, therefore, natural to suppose that this is simply produced by the reflecting action of those scattered particles of which we have before spoken in connection with the source of the sun's light and heat.

Beside the corona there was noticed, as early as 1842, other phenomena, now known by the name of solar flames, prominences or protuberances.

These prominences consist of luminous cloud-shaped masses, of a

* The following portion of this lecture has been re-written since its delivery, on account of the very important discoveries of last August, by which the entire subject was so largely developed, and previous theories so much modified.

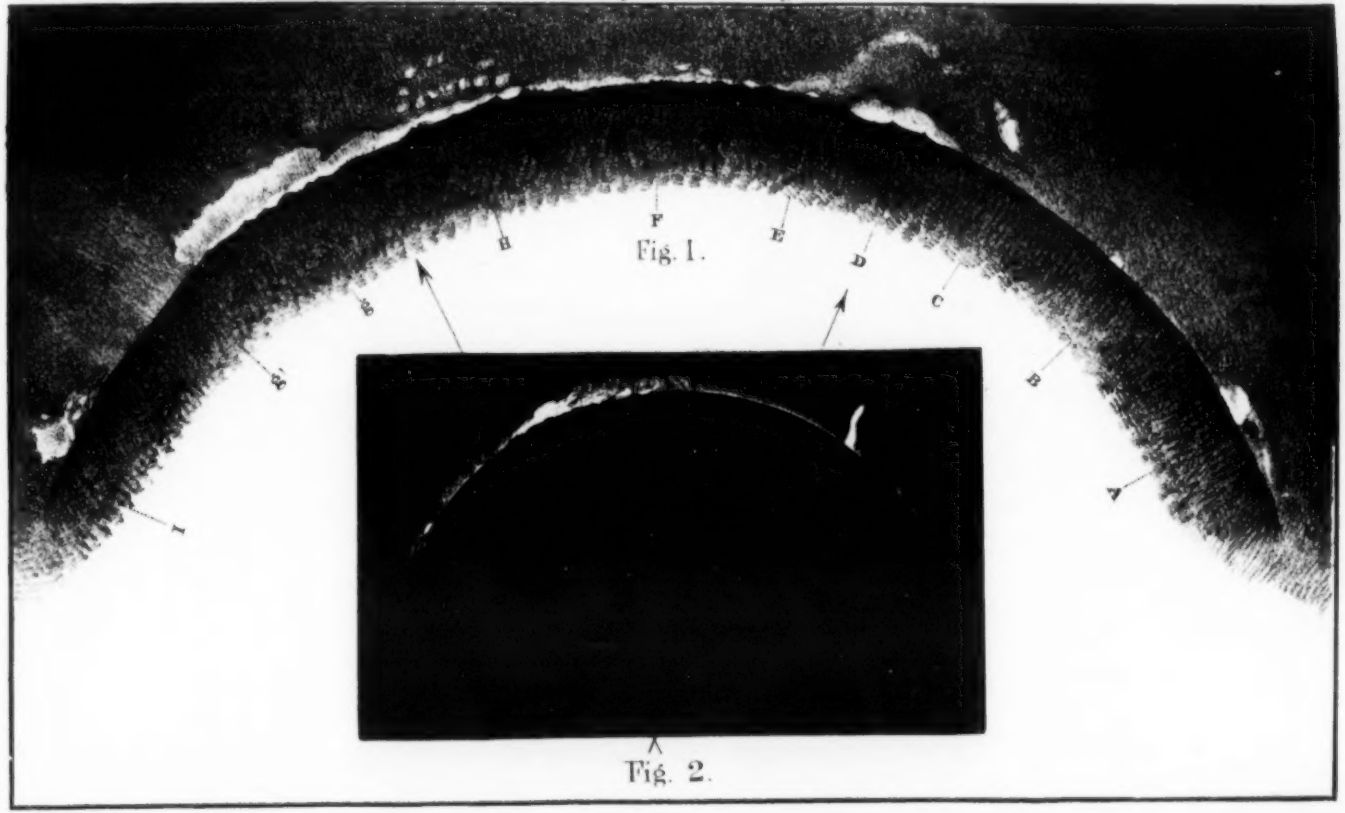


Fig. 1.

Fig. 2.

rose color, in seeming contact with, or floating near to, the solar surface. Figs. 1 and 2 in Plate VIII., will give a good idea of the general forms observed in these objects. Fig. 1 is copied from the fac-simile of an enlarged and touched photograph, published by Mr. De la Rue in the *Philosophical Transactions*, Vol. CLII., part 1.

This engraving shows but half of the original plate, and does some injustice in execution to its original, but it will give a good idea of the character of the phenomena indicated, notwithstanding these drawbacks.

It represents the prominences of the eastern limb of the sun which were by far the most interesting of those recorded on this occasion. The motion of the moon was in the direction of the arrow toward the left. The original negative from which this picture was enlarged was ¾ inch in diameter, taken at Rivabellosa, in Spain, with the Kew heliograph, which had an objective of 3·4 inches diameter and 50 inches focal length, giving a primary image of the sun 0·466 inches in diameter, which was, however, enlarged before falling on the sensitive plate by an ordinary Hygenian eye-piece, to a diameter of 3·8 inches, thus spreading the light about 64 times. The exposure given was one minute, but the negative was so intense as to present great difficulty in printing, and an observation, recorded by accident, during the exposure of the second plate, proved that fifteen seconds would have been long enough. The crooked cloud marked E, and called the "boomerang," and the scattered particles, H, were not visible to any of the observers, although clearly shown in both the photographs which were made on this occasion. (The second plate, twice displaced during the exposure, shows three good images of each.)

The other prominences were of a bright rose and violet color, and that marked C, which was entirely separate from the body of the sun, showed a conspicuous spiral structure. This fact is important in connection with other matters, as we shall presently see.

The weather at the time of this eclipse was very favorable, there being no clouds or watery haze by which either the actinic or visible rays could be arrested, in this respect making a marked contrast with the last eclipse, when at Aden the sky was so covered with clouds that two out of the six negatives exposed were entirely lost, and no trace of "non-visible" details such as were recorded on the other picture were obtained, as might be predicted, for the extreme actinic rays are exactly those which cloud and vapor would arrest.

The brightness of these prominences, under favorable conditions, may be judged from the following data. With the same instrument described above, De la Rue obtained a very faint picture of the moon in three minutes. Judging from the action on the second plate exposed during totality in 1860, an impression of the prominences equal in force, would have been obtained in one second; this would show that these prominences were 180 times as bright as the moon.

Comparing the time of exposure, on the other hand, with that needed to secure a picture of the solar disk, we find that the prominences are 696 times less bright than the solar photosphere.

From all this it would appear that with telescopes adapted to lunar photography, such as that of Mr. Rutherfurd, which makes the picture of the full moon in one second, instantaneous pictures of the prominences could readily be obtained, which would be a very desirable thing, not only because of the number of pictures which might thus be secured during the totality, but also because the errors arising from the change in relative position of the sun and moon during the exposure, could be avoided. This was the cause of some uncertainty as to the true form of several prominences, seen on Mr. De la Rue's picture, on the side nearest the lunar edge. If pictures at all equal to those made by Mr. Rutherfurd, of the moon, and which bear enlargement to a diameter of 30 inches, could be obtained, our knowledge on the subject of the solar structure would be greatly increased.

In connection with this subject of the luminosity of solar prominences, it will be interesting to notice here, that the actual darkness during total eclipse has generally been over-estimated. Thus, De la Rue says that on this occasion, the general light was brighter than the clearest moonlight, so as to admit of reading the micrometer quadrant on the eye-piece, and of drawing without the aid of artificial light.

He further described the light as resembling that of early twilight when stars of the first magnitude are visible, and others becoming so. This, however, was not his impression at the time, but was a determination reached afterwards by examining the drawings, &c., in moonlight and twilight.

The strongly marked indentations along the lunar edge in contact with the prominences, cannot be entirely attributed to actual irregularities in the lunar profile, but were due, no doubt, in most

instances, to the irregular form and brightness of the prominences, in combination with the motion of the moon, by reason of which a bright portion, impressing itself at once, would record its full extent on the negative, while the less intense parts adjacent would be unable to impress the plate until later, when the moon had moved further on, and thus cut off part of their inward edges.

Photographs taken before and after totality by an instantaneous exposure show a serrated edge to the moon, but with much smaller and less deep indentations.

Fig. 2, of Plate VIII., is a copy which in some sort, represents the most interesting of the pictures obtained last August by the North German expedition, under Dr. Vogel, at Aden.

The instrument employed was a telescope, with an objective of 6 inches diameter, and of 6 feet focus, giving an image of the sun ¾ inch in diameter. This objective was ground by Steinheil with special reference to getting the actinic rays in focus, and we may therefore be sure that had the weather proved favorable, pictures could have been taken with the shortest possible "instantaneous" exposure. The exposure actually given, was fifteen seconds, which proves (as well as the accounts of the observers and the pictures after totality, showing the clouds covering the sun) how extremely unfavorable were the atmospheric conditions.

For the various details concerning this eclipse and its observation, we would refer to the article on the subject on p. 268 of our last number. From the spectroscopic observation, made on that occasion and since, there seems little reason to doubt that these prominences are, in fact, cloud masses of very rare and intensely hot hydrogen gas, thrown out from an atmosphere of the same material surrounding the sun (to a depth of about 5000 miles) by some violent atmospheric disturbances, such as we have every reason to look for in this luminary. These prominences have, moreover, been seen to alter in shape, and even to be dispersed within a few hours, and indeed they exhibit all the characteristics of gaseous bodies.

(To be Continued.)

ON THE FUTURE DEVELOPMENT OF SCIENTIFIC EDUCATION IN AMERICA.

By S. Edward Warren, C. E.

Prof. of Descriptive Geometry, &c., in the Rensselaer Pol. Inst., Troy, N. Y.

(Continued from page 285.)

Distribution of Time.—In making a slight comparison of the following programme with others, the aggregate time allowed may seem too large. But it must be remembered that it is one thing to give a brief daily test of a student's preparation of a prescribed lesson, another thing to stand for an hour, and *impart* information to a class, not followed by any test to determine how effectually such information has been *received;* and quite another to conduct a protracted drill upon the varied applications of principles to practice. In nothing, perhaps, is French *training* more in contrast with American teaching than in the thorough drill in the *use* of principles which distinguish the former. It is one thing, for example, to read up, and fairly to understand, Analytical or Descriptive Geometry in five or six weeks, and quite another to be made perfectly at home in them, so far as pursued, by as many months' drilling in the *use* of them.

The following programme, accordingly, contemplates short and numerous lessons, and a force of instruction so large—one professor or tutor to every ten or twelve students—as to secure the utmost thoroughness of drill on practical examples in whatever is undertaken at all.

As every well settled programme of study must *grow*, and not be *made to order*, we have not taken the trouble to compare notes very diligently as to the exact proportion of time which should be devoted to the several subjects in the following scheme. The contingent of fifty hours provides for any mis-adjustments of this sort. It is only set forth as a basis, from which to proceed, till this due proportion can be determined experimentally. Moreover, different educators would differ, probably, more or less, as to what *is* such due proportion. Finally, only a programme in Constructive Engineering is laid down in full, to represent the technical courses. We have then the

GENERAL COURSE—THREE YEARS.

First Year = Third Class.

SUBJECTS.	No. Hrs. on Duty.	Remarks.
Higher Arithmetic	20	With ample practice in examples and demonstration.
Lower Algebra	70	
Elementary Geometry	50	
Mensuration and Elementary Plane Trigonometry	20	
Plane Problems, Theory and Plates	30	
Experimental Physics	60	With practice.
Elementary Geodesy	40	Field practice.
Elements of Geom., Drawing, Theory and Plates.	30	
Projections, Isometrical, etc	40	
Free Drawing, Landscape patterns, etc	35	
Topographical Drawing, Elements	25	
Botany	60	With analyzed collection.
Physiology	40	
History	40	
English	55	With scientific papers.
Other Languages	85	
Contingent	50	
	750	

Second Year = Second Class.

SUBJECTS.	No. Hrs. on Duty.	Remarks.
Higher Algebra	30	With ample practice in examples.
Analytical Trigonometry	30	
Conic Sections and Higher Synthetic Geometry	20	
Analytical Geometry	45	
Higher Plane Problems, Theory and Plates	30	
Descriptive Geometry, with Theorems of Position	85	With construction of plates.
Elementary Perspective	25	
Experimental Physics	55	With practice.
Chemistry, Non-Metallic	60	
Geodesy, with Chain and Compass practice	40	
Topographical Drawing—Farm Maps	35	
Free Drawing	30	
Natural History	50	With collection of specimens.
English	30	With Elocution, etc.
Other Languages	70	
Psychology	40	
Logic	25	
Contingent	50	
	750	

Third Year = First Class.

SUBJECTS.	No. Hrs. on Duty.	Remarks.
Analytical Geometry	20	With practical examples.
The Calculus	85	
Shades and Shadows—Theory and Plates	25	
Perspective	30	
Elementary Mechanics	40	
Experimental Physics	50	With practice.
Chemistry, Metallic and Organic	70	
Geology	40	
Descriptive Astronomy	20	With telescopic examination of the Heavens.
Use of Globes	30	
Construction, etc. of Geodetical Instruments	40	
Colored Topography, Elements	30	
Free Drawing—From Nature	25	
English	45	With composition of scientific essays.
Other Languages	70	
Ethics—Æsthetics	40	
Christian Evidences	30	
Contingent	50	
	750	

Remarks (A.)—Mathematics in General.—In commenting on the composition of a course of study, designed *primarily* for promoting mental discipline, and only *remotely* for securing due professional qualifications; the use of mathematical studies, in the former relation, calls for notice, and invites an attempted answer to Hamilton's celebrated tirade against them. The main point in his argument, as we remember it, is, that as mathematics present a line of truths, each of which *necessarily* follows from the preceding, progress in them is like that of a locomotive on its track, perfectly mechanical; while, as other truth is rather *probable* than *certain*, and interminably ramified, rather than rectilinear, progress in it is more like that of a gymnast through his evolutions, and requires the best efficiency of every faculty. Let us state the case more fairly. *First:* The subject-matter of reasoning is, using Hamilton's nomenclature, of two kinds, "necessary matter"=mathematical truth, in which mathematical reasoning or reasoning on certainties is employed; and "contingent matter," in which "moral" reasoning, or reasoning on possibilities, or probabilities, is employed. Given, then, a mathematical truth. Something really does certainly, inevitably, follow from it. By reasoning, we show *how* the latter

necessarily follows from the former. Again, given a "moral" truth, as the necessary characteristics of slavery, and certain things follow, no less certainly, to the eye of Omniscience, than in mathematical truth. But, to *finite* vision, various results possibly follow. And men reason to show *how* these possible consequences may follow, or may not. In each case, the more immediate the certain or possible consequence of the given premises, the shorter and easier the reasoning process which exhibits their connection; and the more remote the consequence is, the longer and more complex the operation of demonstrating its derivation from that given truth. But the advantages of the two fields of reasoning are not so equal as this comparison might intimate. For, in the first place, it is not true that mathematical reasoning is rectilinear. It is highly ramified or divaricate, as the derivation of the equations, and, thence, all the various properties of the conic sections, from the one "general equation of the second degree," and the numerous ways of demonstrating a given proposition, may abundantly show. Grant, then, that a specific remote consequence follows from a given mathematical truth; for example, from the truth that every polynomial of the *nth* degree, which is an exact *nth* power, is formed by employing some other polynomial *n* times as a factor, it follows that an expression always exists, which, when so taken as a factor, will produce a given expression of that description. Now *which*, of all the more immediate truths, discovered by inspection and experiment and reflection on the structure of the polynomial, is in the line of demonstration that leads to the desired factor where in the line is it, and what follows immediately from it? To cut the matter short, will the speculating philosophers give us a rule for the solution of all equations of a higher degree than the fourth, before they write tirades against mathematics. And, meantime, will they remember, that if mathematics do "conduce to idiocy, madness and death," it may be because they so much more heavily, as well as more worthily tax the powers, than do speculations upon "being in non-being," the "reality or non-reality of time and space," the "perpetual trichotomy of thesis, antithesis, and synthesis," and the absolute Ego, the phenomenal shadow of an ultimate reality, which is our essential self."

The last topic immediately suggests another. If mathematical study consisted only in learning and applying rules, instead of also in *forming* or demonstrating rules, it would be but mechanical

thinking indeed. But, to ascertain the derivation of a rule; to translate the statement of a question, in common language, into its statement by equations; to "discuss" an equation so as to ascertain and interpret all the results which it can afford; and, not least, to transform unusual given equations into forms to which known rules will apply either at all, or most neatly—all this requires a depth and strength of thinking, and a fine and penetrating sagacity, and fertility of invention in ways of proceeding, which constitute an inimitable drill in precise and sharply definite thinking, exactly to the point.

(To be continued.)

Mr. Sandberg's paper on the Manufacture and Wear of Rails published in this *Journal*, Vol. LVI., p. 389 and Vol. LVII., p. 17, was read before the Society of Civil Engineers, London, on the 3d of March, 1868. This statement failed to appear with the paper through an accidental oversight.

Franklin Institute.

Proceedings of the Stated Monthly Meeting, Mar. 17th, 1869.

THE meeting was called to order with the President, Mr. J. Vaughan Merrick, in the chair. The minutes of the last meeting were read and approved.

The Actuary submitted the minutes of the Board of Managers, and reported the donations to the Library received at the stated meeting held March 10th, inst.: From the Institute of Actuaries, the Statistical Society, the Society of Arts, London, and the Association for the Prevention of Steam Boiler Explosions, Manchester, England; l'Ecole des Mines, Paris, and la Société Industrielle, Mulhouse, France; the Water Commissioners, Detroit, Michigan; the Managers of the State Lunatic Asylum, Utica, New York, and Dr. T. S. Kirkbride and Dr. G. Emerson, of Philadelphia. Also, that, on the report of a Special Committee appointed to consider the advisability of, and a plan for a sectional arrangement of the Franklin Institute, the Board of Managers had adopted Regulations for the oganization and Government of Sections, which were therewith submitted.

In accordance with a previous resolution of the Institute, the following memorial was then read by Prof. Fairman Rogers:

BIOGRAPHICAL NOTICE OF PROF. ALEXANDER DALLAS BACHE.

ALEXANDER DALLAS BACHE, the subject of our memoir, was born in Philadelphia, on the 19th of July, 1806. His father, Richard Bache, was a son of Richard Bache, Postmaster General of the United States, and Sarah, only daughter of Benjamin Franklin. His mother, Sophia Dallas, was the daughter of Alexander James Dallas, Secretary of the Treasury, and the sister of George M. Dallas, Vice President of the United States and Minister to London.

Mr. Bache was thus connected with families alike distinguished for scientific and intellectual attainments and social position, in his native city.

In 1821 he entered the United States Military Academy at West Point, and graduated at the head of an uncommonly good class in 1825, remaining for about a year after his graduation as assistant instructor in engineering. He was then assigned to engineer duty at Fort Adams, Newport, R. I., where he remained two years, with General, then Colonel Totten, and while there became engaged to Miss Fowler, to whom he was married in 1828, a woman adorned with those good qualities which enabled her to be his firmest friend and most able adviser throughout his life. In 1828 Mr. Bache was appointed Professor of Natural Philosophy and Chemistry in the University of Pennsylvania, at the age of twenty-two, and it is from that time that his scientific career commences. In 1836 he was appointed the first President of Girard College, and made his trip through Europe in the interests of that institution. Upon his return, finding that much time must yet elapse before the College buildings could be finished, he offered his services to the city to re-organize its High School, and at the end of a year, the College not yet being in a condition to require his attention, he resigned his salary, but retained the office of President, holding himself ready for duty when the building should be in readiness, and became Principal of the High School and Superintendent of Public Schools, for which he received a salary from the city. He again took his old chair in the University in 1842, and in 1843 he was appointed Superintendent of the United States Coast Survey, which position he held until his death, which occurred on the 17th of February, 1867.

Such is a brief record of one of the most useful and brilliant lives of the past half century. His devotion to science, in the highest

acceptation of the term, knew no bounds. His enthusiasm not only carried him forward, but it communicated itself to all who came in contact with him. One reason that he did so much work, was that he was hundred-handed. The moment that he made the acquaintance of a new man he saw what he was good for, what he could do, and, by some mysterious power, he set him to work. When he proposed a line of research, or a matter to be worked up, it always seemed to him to whom he proposed it, that here was just the opportunity for which he had been waiting, and he attacked it with vigor and determination. Thus the great chief always had many hands and brains occupied with the details of the matters which were at the time interesting him, and these outside labors met with so much consideration, and were so handsomely acknowledged, that no one hesitated to repeat them when called upon a second time. No scientific man could render a greater service to science than by encouraging its younger votaries to take up and to persevere in those investigations for which nature may have fitted them, and in this Mr. Bache was eminently successful. He showed this conspicuously when, even as a young man, he was interested in the operations of this Institute, and it is in his labors as one of our old members that we are especially interested.

Very shortly after his appointment to the University, Mr. Bache connected himself with the Franklin Institute. On the 25th of March, 1830, his name first appears in the records of the Society, as chairman of the monthly meeting of that date, and from that time until his departure for Washington, in 1844, he was a prominent member, serving faithfully on most of the important committees. He was in the Board of Managers from 1831 to 1839, and Corresponding Secretary from that time until 1843. He served for many years on the Committees on Meteorology, Inventions, Instruction, Meetings, and Publication, and on the Committees on Patent Laws and the Manufacturing Establishments of Pennsylvania. The Committee on Inventions was extended into the Committee of Science and the Arts in 1834.

He was appointed on the Committee on Explosions of Steam Boilers and Strength of Materials, in June, 1830, and labored faithfully as a member of both its sub-committees, taking a large part in the preparation of the report, which is still considered one of the classical authorities on the subject. He also took a large share in the labors of the Committee on Experiments on Water Power. As a member of the Committee on Weights and Measures, in 1833, he

took a large share in the preparation of the valuable report on that subject. In October of 1842 he delivered the address at the close of the Annual Exhibition. During all this time he contributed many papers to the *Journal* and to the Philosophical Society, of which he was an active member. He edited an American edition of Brewster's Optics, and conducted a large number of investigations in Magnetics, Meteorology, and Physical Science generally. This was probably the time of the greatest activity of the Institute, and Mr. Bache had associated with him as fellow-workers, and as staunch friends for all after life, S. V. Merrick, Frederick Fraley, Dr. Hare, M. W. Baldwin, T. U. Walter, Sears C. Walker, J. C. Cresson, J. F. Frazer, men whose names we now honor as among the brightest lights of the Society.

In 1843, Mr. Bache having received the appointment of Superintendent of the United States Coast Survey, left Philadelphia, and, of course, his active duties in the Institute, to take up his residence in Washington, but he always considered this city as his home, and looked forward to returning to it at some future period, when his labors for the Government should be ended; and one of his greatest pleasures was to meet his old associates on the occasions of his frequent visits to Philadelphia, and talk over the times when they worked together for the interests of the Institute and of science.

When Professor Bache took hold of the Survey, he found himself in a position which required all his tact to make comfortably tenable. Some of the older assistants felt aggrieved that a person hitherto unconnected with the work should have been selected as its head, and for many months there was a disposition to make things go roughly, which might have disheartened a man who had smaller views, or less disposition to seize upon the opportunity afforded him to make his new work one of the grandest contributions to the science of the age.

Extreme firmness, imperturbable good humor, and a manner which made all who approached him friends and totally disarmed his enemies, finally enabled him to overcome all obstacles in the interior of the survey, and he applied himself with all his energy to the elaboration of the organization, and the introduction of all the best scientific methods, most of which he extended in their practical application, to a point not before reached.

He soon enlisted the best scientific power of the country, either as officers of the Survey, as temporary assistants for some special

work, or as friends, who, for pure love of the man and interest in a work to which he devoted his energies, were always ready to contribute their advice or co-operation in those matters which belonged specially to their line of study. It was in this way that he won the title of "Chief," applied to him by a large and ever increasing circle of scientific men, who appreciated him as the leader of organized science in America.

His peculiar position gave him advantages which could hardly be enjoyed by any other scientific man. Visiting each seaboard city frequently in the discharge of his official duties, he was constantly in personal contact with his acquaintances, and had every opportunity of seeing the new men who grew up in each place. Accustomed to the details of commercial and political business, he had much broader and more practical views than those which are sometimes the result of seclusion in the study or the laboratory. The fact of his having graduated with all the honors of the Military Academy placed him upon a footing with the officers of the army and navy, which was of the greatest advantage to him in his connection with both branches of the service. It has always been usual to detail some army and navy officers for duty upon the Survey, and it not unfrequently happened that Bache had to administer one of his quiet reproofs to some young officer, who, forgetting, or perhaps ignorant of the fact, that his Chief was a regular army man, would attempt to plead a "custom of the service," or a point of etiquette, as an objection to some distasteful duty. Of that rare power of administration which appears to be partly natural and partly the result of education, Mr. Bache had a large share, and the scientific and business operations of the Survey moved like clock-work under his guiding hand. He loved to put the machinery together, wind it up, and then dismissing it all from his mind, hear the report at the end of the month or designated time, when he would take up the thread of the matter just where he had last left it, as if he had thought of nothing else during the interval. He understood thoroughly the way of doing nothing for himself that could be done for him by others, and thus reserved his time and his powers for that work which he alone could do. His practical knowledge of methods of observation was extrordinary, and he could pick out interpolated figures in records of work, or tell an astonished observer that on such a night he had omitted to examine the level of his instrument, with an accuracy that bordered upon the marvellous.

His capacity for work was astonishing. Not contented with the large and ever increasing labors of the Survey, he was an active member of the Board of Regents of the Smithsonian Institution; associated there with his warmest and most trusty friend, Professor Joseph Henry, of the Light House Board, of numerous special boards on harbor improvements, President of the National Academy of Sciences, and ready at all times, and constantly called upon, to use his tongue or his pen to advance the true interests of science at all points.

It is not common that with those abilities to which we have referred are combined those social qualities which render their possessor agreeable in ordinary life, but Mr. Bache possessed them in an eminent degree. Released from his official duties, about which he was usually very methodical, he was the pleasantest companion at the dinner table or in the saloon, that young or old could desire. Extremely fond of society, his hospitable house in Washington was always open to his friends, who carried away with them the most charming reminiscences of its bright wood fires and sparkling candles, and in his summer camps there were always some extra tents for those who were fortunate enough to receive invitations to visit him in his wild retreats. He spent several months of each year under canvas, at the primary triangulation stations, or on base measurement, and returned to his duties in the capital refreshed and invigorated by the mountain air, long strolls, and change of scene. Bright reminiscences are those of these mountain camps, with the morning's writing, the midday dinner, the genial face of the kind hostess, the pleasant chat over the bottle of Rhine wine, and, if there was no observing in the afternoon, the long rambles down the hill, with the climb back again, the camp being of necessity very near to the summit, finishing up with an evening of conversation or reading, unless the stars were good enough to allow themselves to be observed.

With a never flagging determination to carry the scientific operations of the Survey up to and beyond the highest point of excellence attained in other countries, Mr. Bache spared no pains or thought in perfecting all the details of the various processes with such success that in every branch important steps were made. To the apparatus for the measurement of bases especially, as being the instrument upon which the accuracy of succeeding work depended, he early turned his attention, and produced a base measurer which

is yet without its equal in the world. In 1845 he took up the subject, and discarding the principle of using surface marks or dots on the measuring bars, and bringing them into coincidence by means of microscopes, he applied the contact level already invented by Repsold for another purpose, and by thus introducing the method of end contact, facilitated the comparisons with the standard bars, the practical working in the field, and the accuracy of the operations in a remarkable degree. The extremely beautiful method of equalizing the conducting power and consequent rapidity of expansion of the iron and brass bars of the apparatus, by making their sections proportional to their conducting powers and specific heats, and then making the final and most delicate correction by applying varnishes of different colors, is an admirable example of the care which he bestowed upon the smallest details.

Having found the metrical system in use on the Survey as introduced by Mr. Hassler, he continued it, and always felt a deep interest in the adoption of that, or of some other universal system of weights and measures, by the civilized world. His position as Superintendent of Weights and Measures, of course, brought the subject constantly under his notice. As a member of the Committee on Weights and Measures of the National Academy, he also discussed the matter thoroughly, and at one time he leaned evidently towards making an attempt to establish, by a Congress of nations, an *entirely new* standard, which should be adopted as an universal one. His two general objections to the metrical system were: first, the fact that later observations have shown that the metre is not the 10,000,000th part of the earth's quadrant; and secondly, that the actual length of the metre is not, in practice, nearly so convenient as that of the foot or the ell. The latter is no doubt the strongest objection that can be made to the metre as an universal standard, and perhaps the only one; and later, Mr. Bache seems to have determined that the metric system had too strong a hold to be rooted out by any other where it already has been adopted, and was prepared to give his unqualified support to any measures looking to its adoption as an universal system. It is a little singular that the period of his death witnessed the legalization, to a limited extent, of the metric system in England and the United States, and the consummation of one of his most cherished projects, the determination, by the Atlantic Telegraph Cable, of the difference of longitude between Greenwich and Washington, which was

made for the Coast Survey in December of 1866, by Dr. B. A. Gould. For several years, in fact since the laying of the original cable, everything had been prepared for these observations, and the preliminary report of their success was made to the National Academy of Sciences in January, but one month before his death.

During the rebellion Mr. Bache threw himself, heart and soul, into the service of the Government, and took a most arduous part in the labors of the blockade commission, at the very outset of the war. His judgment and far-sightedness enabled him to withdraw his parties and vessels from the South almost without a loss, and no information of any value fell into the hands of the rebels from Coast Survey sources. The Survey furnished most valuable officers during the war for military surveys, and they were much needed, since the engineer officers, being all graduates, were rapidly promoted to line appointments, and technical knowledge in that branch of the staff was sadly needed.

In 1861 Mr. Bache became Vice-President of the Sanitary Commission, and was throughout the war a most influential member of that imporfant body. When Lee threatened Philadelphia in the Gettysburg summer, Mr. Bache did not forget his native city, but immediately offered his services, and those of the officers he had near him, to make a military reconnoissance of the vicinity of the city, and to locate works, to be built if the necessity required. In that sultry summer weather he worked, literally, day and night, and exhausted a frame never much accustomed to severe bodily exercise, and it is doubtful whether he ever recovered entirely from the effects of the labor and worry which he underwent at that period.

When the National Academy of Sciences was established by Congress, in 1863, the choice for President fell, without dissent, upon Mr. Bache, and he continued to discharge the duties of the office with the greatest energy and judgment until his illness withdrew him from active labors. His guiding hand, his moderate counsels, and his constant vigilance in seizing upon every turn which could be of advantage to the scientific usefulness of the Academy, were of the utmost importance to its welfare and power. In the spring of 1864 his health began to fail him. Too much intellectual labor had done its work, the body was too weak for the mind, and a long summer rest, and finally a trip to Europe, were prescribed as the cure. Temporary relief, however, was all that these